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NOTES AND COMMENTS

The Confirmation of Darwin's Theory of Coral Islands

During the past few years there has been a lull in the long controversy regarding the origin of the coral atolls of the Pacific. The arguments for and against Darwin's theory had been so often repeated that people were tired of deductive reasoning on a problem which might at any time be solved by a practical test. It has been admitted for many years that the only method of ending the controversy is to make a deep boring in an oceanic atoll. The first attempt to apply this method was made during the expedition of the United States cruiser, the *Tuscarora*, but the boring tools broke at a slight depth. In 1891 a committee was appointed by the British Association to arrange a plan for a new attempt. The Admiralty was willing to assist by help of a surveying ship, and the Government of New South Wales was ready to lend the boring equipment. A report was prepared, and it was recommended that Funafuti, one of the Ellice Islands, should be the site of the experiment. Accordingly a joint expedition was sent out by the Royal Society and the Australian Museum at Sydney, and was led by Prof. Sollas. Valuable natural history collections were made at Funafuti, and many interesting anthropological observations recorded; but the main object of the expedition was not accomplished, as the boring failed. The observations collected were claimed by Admiral Sir W. J. L. Wharton as opposed to Darwin's theory; whereas Mr Chas. Hedley of the Australian Museum maintained that the general survey of the island strongly supported it. With splendid perseverance the Australian authorities resolved to renew the attempt. Accordingly a new expedition was sent out under the command of Prof. T. Edgeworth David of Sydney. A telegram from Sydney on October 4 announced that all the difficulties had been successfully overcome, and that the boring had been carried to a depth of 557 ft., and was still in coral rock. A further telegram on October 12 announces that the depth of 643 ft. has been reached and the boring is still being carried through coral rock.

Prof. Edgeworth David may therefore be congratulated on having finally proved conclusively the truth of Darwin's brilliant theory. It may be objected that it is too soon to shout, as the cores have not yet been subjected to microscopic examination, and that one boring is not sufficient. But neither objection is worth much. Coral-reef rock is of very varying composition; the coral grows in hummocks separated by more or less narrow spaces, which are filled up by coral sand, broken shells, foraminifera, &c. Microscopic examination of fragments of limestone broken from coral reefs sometimes shows no trace of coral. Coral, moreover, is more readily decomposed than shell sand or foraminiferal limestones. Hence it will not be surprising if most of the slices cut from the deep core shew no trace of coral structure. But that will not prove that they are not to be regarded as reef rock. Nevertheless it is to be hoped that some parts of the limestone from the bottom of the core will be sufficiently well preserved to show the nature of its formation.

To the objection that one boring is not sufficient to prove a theory proposed for so many islands, it is only necessary to point out that Darwin himself insisted that his theory was not universal. He never intended it to apply to the coral reefs of the West Indies, the Red Sea or the Persian Gulf. What the Funafuti boring has proved is that the subsidence method is possible; and if one island in the Ellice Archipelago is sinking, it is probable that the other islands in that group are also doing so. No doubt some cases will be found in the Pacific of coral islands formed on banks left by denuded volcanoes. But the arguments which Darwin used to show the improbability of many of the atolls having been formed in this way are still valid; and they are strengthened by the demonstration in the only atoll yet tested, that the basis is a block of subsided reef limestone.

There is some zoological evidence in support of a former migration of land animals across the area now occupied by the coral sea; and naturalists are now at liberty to explain their distribution by the former existence of land in that area.

The persistence with which the Australian naturalists have persevered in their attempt to settle this controversy, and the skill with which Prof. Edgeworth David has overcome the mechanical difficulties, are worthy of the highest praise.

The Temperatures of Reptiles, Monotremes, and Marsupials

Several other interesting contributions have reached us this month from the Antipodes, and among the most valuable may be noted an account of some new experiments on the body-temperature of verte-

brates by Mr Alexander Sutherland (*Proc. Roy. Soc. Victoria*, n. s., vol. ix., pp. 57-67, pl. vi.). For many years past there has been a tendency to diminish or ignore the distinction between the cold-blooded and the warm-blooded types of animal life. The new results, however, seem to confirm the idea that the distinction is a real one, though they show more clearly than ever that several gradations between the two types still survive in the existing world. Cold-blooded animals sometimes develop a capacity for heat-production in the action of their viscera. Mere digestion, for example, may increase the temperature of a snake from 2° to 4° C., while amatory emotion is known to have the same effect on snakes, lizards, and frogs. But in general this excess of warmth is not great, and it leaves the gap between the warm-blooded and the cold-blooded type quite evident.

Mr Sutherland's first two experiments were arranged to re-determine to what extent the temperature of a reptile varies with that of its surroundings. He placed some lizards in a tank of water, leaving only their noses uncovered, and then warmed the water at various rates of speed by means of one or more lamps. In each case he found the rise in temperature of the animal and of the water to correspond almost precisely. Other observations also point to the same conclusion, namely, that cold-blooded animals at rest take their temperature almost absolutely from their environment.

Mr Sutherland next records his experiments with the Monotreme Mammalia. Their low temperature has often been remarked upon. Baron Miklouho-Maclay once determined that of the duck-billed platypus (*Ornithorhynchus*) to be only 24·8° C., while the average temperature of forty-five specimens of the higher orders of the mammalia (excluding monotremes and marsupials) observed so long ago as 1825 by John Davy, was proved to be nearly 39° C.—a result subsequently confirmed by Max Fürbringer. The platypus is, indeed, almost a cold-blooded animal, and the echidna rises very little higher in the scale. Mr Sutherland finds the average temperature of *Echidna hystrix* to be 29·4° C., but it curiously varies:—"An echidna one cold morning was so low as 22°; another, brought in from the forest in a sack exposed to a fierce midday heat, registered so high as 36·6°. . . . This is an immense range for a mammal, and suggests a reptilian want of capacity for temperature regulation."

As the result of 126 observations, Mr Sutherland determines the average temperature of sixteen different species of marsupials to be 36° C., or three degrees below the average of the higher mammals. The marsupial most nearly approaching the monotremes in temperature proves to be the wombat (34·1° C.). Next comes the flying squirrel (*Petaurus*), with average 35·7° C. Eighty-three

observations on the koala (*Phascolarctos*) show its average temperature to be 36·4° C. The range in any one individual at different times is generally proved to be very small. Mr Sutherland, however, observes:—"I have often known healthy specimens (of *Phascolarctos*) that had been for a while in the sun stand as high as 37·9°, while on a cool day, or in a very shady place, the same individuals would be only 35·3°, a range greater than we would find under the same circumstances in any of the higher mammals. The highest register I ever obtained for a thoroughly healthy koala was 38·4°, which is a degree and a half above the normal temperature of man; the lowest was 34·9, or nearly two degrees below man's normal. The former temperature would in man imply some constitutional derangement, a distinct case of feverishness; in the koala it denotes only that it has been out in the sun. The lower temperature, though common in the koala, is never met with in man except in rare pathological conditions."

It is clear, therefore, that there are grades of temperature, and that the mammals which are classed lowest on anatomical grounds are not only of the lowest temperature, but also of the greatest range, and they are likewise, of all mammals, those which are under the strongest and most direct influence of the temperature of the environment. Similar, though more incomplete, connecting links may also be noted in the case of birds.

In a very general way (concludes Mr Sutherland), and not forgetting numerous limitations and contradictions, it may be said that bodily activity depends on body temperatures, that creatures, such as insects and reptiles, are active only when warmed up from without, but become torpid with decreasing temperature. The type in which activity is generally habitual, maintains its own body temperature. This is seen in the mammals, but more still in the birds. But this warm-blooded active condition was produced by no sudden emergence; the monotremes and marsupials form a gentle gradation between the reptile and the carnivore or ungulate; while, so far as indications point, there is reason to believe that the lower birds still are reminiscent of a once existent chain of links which equally joined the cold-blooded lizards to those warmest-blooded of all creatures, the passeriform and fringilliform birds.

Entomology in Australia

Mr W. W. Froggatt has published (*Proc. Linn. Soc., N.S. Wales*, 1896, pp. 510-552) the second part of his work on the termites of Australia. A short account of the life-history and social economy of the insects, unfortunately written without reference to Grassi's recent researches, is followed by a revision of the genera, which

will be valuable to naturalists elsewhere than in Australia. The Australian species which have come under observation are carefully described and figured.

In addition to such systematic work, Mr Froggatt turns his attention to economic problems, and we have received several of his entomological notes from the *Agricultural Gazette of New South Wales* for the current year. Some of the insects dealt with are native species, which have become destructive in gardens and orchards, while others are familiar British forms introduced with European plants.

The Cuckoo

The nesting habits of cuckoos are always interesting, and Mr A. J. Campbell has recorded in the *Victorian Naturalist* for August a list of the foster-parents of the Pallid Cuckoo (*Cuculus pallidus*, Lath.) of Australia. The observations show that the cuckoo almost always selects open nests, and that the Honey-eaters are the most favoured foster-parents. Mr Campbell mentions that the supposition that the cuckoo throws out the egg or eggs of the foster-parent to make room for its own has not been proved with regard to the Pallid Cuckoo; but he has found a broken egg of the bird underneath the nest of the White-shouldered Caterpillar-catcher (*Lalage tricolor*). At present there is no record of any cuckoo's egg having actually been taken from any Caterpillar-catcher's nest, though one of these birds has been observed feeding a young Pallid Cuckoo.

The Mental Development of a Child

The latest of the series of larger monographs published by the editors of the *Psychological Review* is a careful record by Mrs Moore of the behaviour of her boy during his first two years. Such records by psychologically-instructed observers are likely to be of much use for child-psychology; and this may be said of the present one, though the want of arrangement in the earlier part detracts seriously from its value. Great masses of facts are bewildering unless arranged on some principles. The writer begins very suggestively by distinguishing four periods—first, of seeing till the end of the fourth month; second, of feeling or fingering things till the seventh month; third, of examination or more systematic exploration; fourth, of speaking from the close of the first year. Unfortunately, this division does not reappear in the body of the work. The classification of movements is also confused. But having said this, we may be grateful to Mrs Moore for what she has given us; and it will be best here to select a few of the interesting points which occur in the course of the work. The first is a notice of

certain epochs or periods in which the child made very rapid progress, on the thirty-fifth day, in the eleventh month, etc. (pp. 6, 7) — an important observation on which more data are desirable. Under the head of movements (Part I.) there is a useful record of voluntary movements, pointing to the growth of volition out of "repetition of an act which had originally caused either a cessation of discomfort or a sense of gratification" (p. 27). Many of the conclusions drawn are naturally rather corroborative of current psychology than pointing in any fresh direction. Parts II. and IV. are inferior in quality to the rest. Under Part II., which deals with Sensations, we have, on pp. 45 and 56, the note that even on the second day the eyes followed the movement of a bright pair of callipers—a date important on its bearing on the theory of visual space-perception. On page 66 we have the less important denial of an unborn ability of localising sounds. On pp. 80, 81, there are some remarks on the localisation of pain, which is declared to follow that of "other" dismal sensations. On page 87 it is denied that attention, at any rate in its involuntary form, comes late: the child gazed at a patch of light continuously on the thirtieth day (p. 46). Part IV. deals with language, giving careful tables of the sounds used and the principal substitutions of sounds for one another, as well as full vocabularies of the child at the close of the second year. The order at which the different parts of speech begin to appear is noted, as well as their numerical importance. One point which is emphasised more than once (pp. 123 and 97) is that the child's first names do not refer to indefinite or vaguely conceived individuals, but that the child does not understand the necessity of a name for each separate thing, and his words stand for what is interesting to him in his experience. As against any idea that general concepts arise from the fusion of individual precepts—this is however what would generally be understood. Mrs Moore's work will be a useful repertory of facts, to which she has been careful to supply an index.

The International Geological Congress

The Seventh Session of the International Congress was held in St Petersburg last August with great success. The attractive programme offered by the Russian geologists, with the aid of their Government, brought together a large number—nearly a thousand—especially from Germany and Austria. Americans and French were well represented, largely by mining engineers anxious to study the rich ore deposits of the Ourals. Englishmen were no doubt diverted to the other side of the Atlantic by the meeting of the British Association, and were therefore proportionally few.

Of the excursions before the Congress, those to Finland and

Esthonia seem to have been more successful than that to the Ourals, owing to the difficulty of transporting the larger number in the latter case. Those who visited Finland had an opportunity of testing the classification of supposed Archaean rocks recently put forward by Mr J. J. Sederholm (see *Natural Science*, vol. x. 1897, p. 79). The general opinion seemed to be that the evidence was insufficient to warrant the ascription of an Archaean age to many of the rocks. Under the leadership of Baron de Geer the glacial phenomena of the district were studied, and the Åsar, with which many English geologists made their first acquaintance, gave rise to interesting discussions. The main object of the Esthonian excursion, under the direction of Akademiker Friedrich von Schmidt, was the examination of the Cambrian, Ordovician, and some of the Silurian rocks of the Province. The junction of Ordovician and Silurian was not over easy to follow under the conditions of the excursion, but the grand series of absolutely unaltered Cambrian with *Platysolenites*, *Olenellus*, *Obolus*, and *Dictyonema*, was a revelation to many. The party had also the advantage of the presence of A. von Mickwitz, whose palaeontological work in these Cambrian beds has been of the highest value.

At the Congress itself, if no practical results were arrived at, still many discussions on matters of general importance to geologists took place. The main problem put before the Congress was the classification and nomenclature of rocks, both stratified and igneous, but chiefly the former. Treatises by Messrs Frech, Bittner, Walther, and Loewinson-Lessing formed the basis of discussion. This resulted in the following resolutions: "The Congress is of opinion that the historical method of classification must be adhered to, though it should continually be made more natural. The council is to nominate a committee to study the principles of classification in this spirit." "The introduction of a new stratigraphic term into international nomenclature should be based on a clearly defined scientific necessity supported by peremptory reasons. The appellations applied to a terrane in a definite sense cannot be applied in any other sense. The date of publication is to decide the priority of stratigraphic names given to the same series of beds." "For the minor stratigraphical divisions, sufficiently characterised palaeontologically, in the case of the creation of new names, it is preferable to take as their basis the most important palaeontological characteristics. Geographical or other names should only be used for divisions of a certain importance containing many palaeontological horizons, or when the terrane cannot be characterised palaeontologically." "Names badly formed from an etymological point of view are to be corrected without excluding them from the domain of science." Certain proposals of minor importance were referred to the above-mentioned committee.

There is no doubt that with the advance of geological knowledge

we suffer from a superabundance of names. But it is to be hoped that the promulgation of these rules will not call into existence an army of nomenclaturists, whose delight it will be to search antique literature for forgotten appellations, or to puzzle their brains with lexicons in the attempt to make all names pass the same etymological standard. We have enough of this sort of thing in systematic biology, and we don't want any more of it.

After the Congress about half the members took part in the various excursions that culminated in the Caucasus and the Crimea. The Carboniferous rocks of the Moscow neighbourhood, the Permian of Nijni-Novgorod, the Mesozoic and Cainozoic strata of the Volga and Donetz basins were introduced to the foreign geologists by such able students of them as S. Nikitin, A. Pavlov, W. Amalitzky, T. Tschernyschev, and N. Sokolov. The difficulties of transporting and personally conducting such large numbers were successfully overcome, but necessitated the compression of the passage of the Caucasus into far less time than was desirable or was originally intended. Side excursions to Elbruz, Grozny, Astrakhan, the Tsei and Devdorak glaciers, and other points of interest compensated for this in some measure, and usually proved to be the most enjoyable parts of the excursion. After ridding themselves of superfluous roubles in the bazaars of Tiflis, the visitors explored Baku, the city of petroleum, and then crossed the Black Sea to study the Jurassic, Cretaceous and igneous rocks of the Crimea under the leadership of A. de Lagorio, N. Andrussov, C. de Vogt, and others. F. Loewinson-Lessing, after his arduous Caucasus campaign, engaged in the no less difficult enterprise of transporting some forty persons, of both sexes, to Ararat. With the rough way made plain for them and the crooked straight, those who took part in this excursion declared that in beauty and interest it was worth all the rest put together. Its close was unfortunately saddened by the loss of Mr Stoeber, a lecturer on pharmacy at Vladikavkaz, who was helping Professor Lessing. Joining in an unsuccessful attempt to ascend Great Ararat, he was more rash than his companions, who subsequently found him frozen to death.

The pleasure of many of these excursions was seriously marred by the great numbers that availed themselves of the exceptional opportunity. Worse still, it appeared that many of the members were hardly geologists. An attempt will probably be made in future to restrict the membership of the International Geological Congress to recognised workers. There is no reason why such a body as this should have its dignity and usefulness marred by all the tag-rag-and-bobtail that choose to rush for railway passes and free champagne, and we shall warmly support any movement for the more sparing distribution of its privileges.

A Floating Scientific Station

An important proposal was laid before the International Geological Congress by Professor Andrussow. It was that a ship, fitted with scientific laboratories and apparatus, should constantly be maintained at sea by international contributions, and that geologists and biologists of all contributory nations should be allowed a place on board for carrying out observations. The importance of the study of the ocean-floor, and of all marine deposits now forming, will be denied by no geologist, but the opportunity as a rule is lacking. Hence geologists no less than biologists are interested in the maintenance of such a floating scientific station. The difficulties in the way of the proposal are too obvious to need comment, but if there is a real desire to see it put into effect nothing need prove insuperable. Since the idea received the warm support of Dr John Murray, Professors von Zittel, Haeckel, Walther, Prinz, and other influential scientific men, there is no doubt but that we shall hear more of it, and we wish it all success.

The Black Sea

An excellent illustration of the geological value of thalassography was afforded by the Black Sea. On the steamer that conveyed the main body of geologists from Batoum to Odessa, dredging apparatus was provided and a small laboratory fitted up, enabling those who wished to verify for themselves the interesting account of this sea given by Professor Andrussov in the Livret Guide. The most striking peculiarity of the Black Sea is the absence of all life except bacterial at depths exceeding 100 fathoms. The cause of this may be put briefly thus. Into the deep and steep-sided Euxine basin there is poured, especially on the northern side, a vast amount of cold fresh water from the rivers. Thus there is started in the direction of the Bosphorus a surface stream,

> "Whose icy current and compulsive course
> Ne'er feels retiring ebb, but keeps due on
> To the Propontic and the Hellespont."

From the Bosphorus into the Black Sea a very slow under-current brings the warmer but salter and therefore heavier waters of the Aegean. These scarcely mingle at all with the surface waters, but sink to the bottom. The exchange takes place so slowly that, according to the calculations of Admiral Makarov, it requires at least 1700 years to renew the water of the lower strata, whereas the water of the upper 100 fathoms is renewed annually. Below 100 fathoms, therefore, the quantity of oxygen contained in the water diminishes rapidly, and while the cold brackish water of the surface and of the narrow shelving shore is unfavourable to the development of ordinary marine life, the deoxygenated water of the depth, despite its saltness and warmth, is absolutely fatal to all organisms other than bacteria.

The action of these deep-sea bacteria is truly remarkable. In other seas the rain of dead organisms from the surface plankton forms the food of other organisms living in the depths. But here the rain, after it has fallen through the upper 100 fathoms, finds no organisms to eat it. The microbes have it all to themselves. The albumen of the descending dead organisms putrefies under the influence of the bacteria; oxygen is taken from it to form carbon dioxide, and hydrogen sulphide is evolved. The carbon dioxide appears to help in the formation of the fine precipitate of carbonate of lime that is found in the depths. The hydrogen sulphide partly acts on the salts of iron in the water, forming iron sulphide, partly decomposes as it reaches the oxygen of the surface.

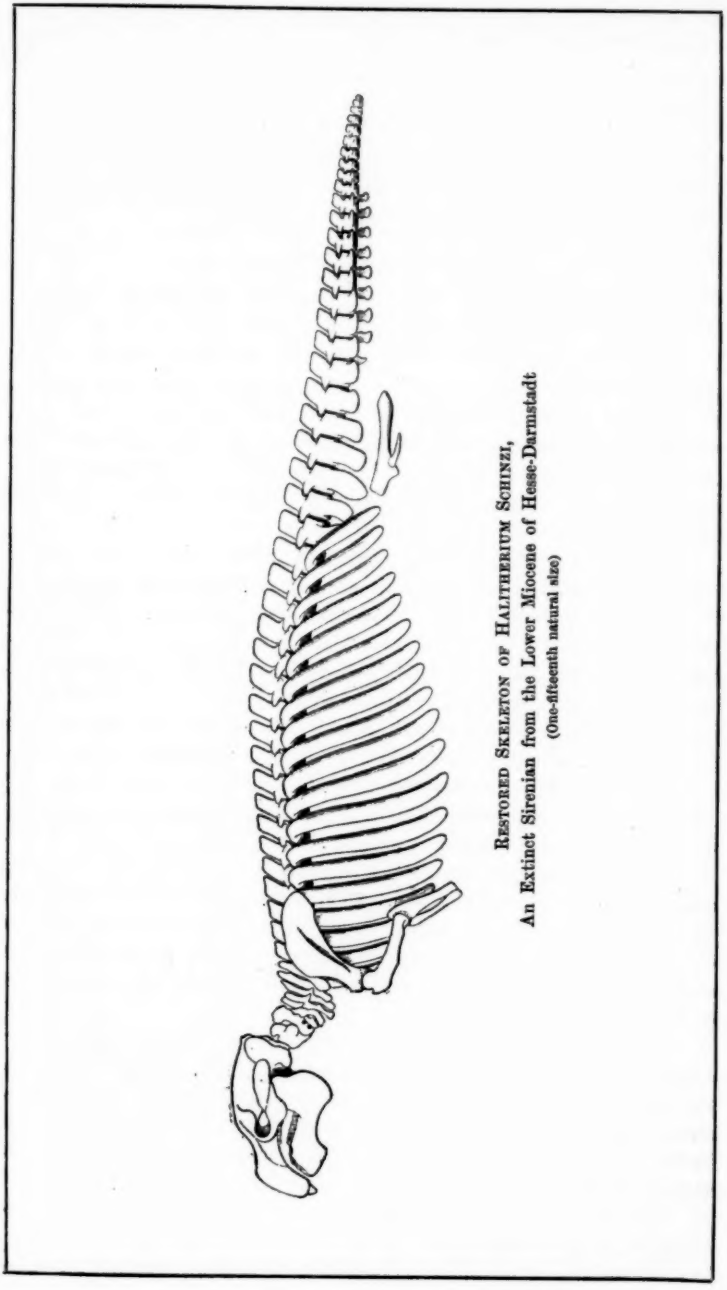
The gradual establishment of this peculiar state of things can be traced. Geological evidence shows that in Oligocene and Miocene times the Euxine and Caspian basins were connected, only being separated by the final upheavals of the Caucasus. Connection with the Aegean was due probably to the cutting down and lowering of a river-channel, of which the Bosphorus and Dardanelles are the remains. When this took place is uncertain. It cannot have been long ago, geologically speaking, because the shells of *Dreissensia* and other brackish-water molluscs are found lying on the bottom of the Black Sea at depths where neither they nor any animals can now exist. On the other hand the northern character of the Black Sea fauna, notably the presence of the common porpoise, suggests that the connection existed already during the Glacial Period.

Further interesting details, together with a description of the deposits now forming in the Black Sea, will be found in Professor Andrussov's guide. We have merely quoted enough to show the intimate relations between geology and oceanography, relations which we are glad to see officially recognised by the International Congress.

An Extinct Sea-Cow

Last month (p. 223) we briefly referred to our unfortunate lack of knowledge of the ancestors of the Sirenian mammals commonly known as sea-cows. In reference to this subject, we have now been favoured by Mr A. S. Woodward, of the British Museum, with the accompanying restored drawing of the skeleton of the best-known extinct Tertiary Sirenian, *Halitherium*, which he has recently had prepared for a forthcoming work on Vertebrate Palaeontology. This figure (p. 299), which is of about one-fifteenth the natural size, is mainly based upon a skeleton in the Museum of Mayence and upon the researches of Dr G. R. Lepsius, of Darmstadt.

Halitherium schinzi is found in the Lower Miocene sands of Hesse Darmstadt, and so dates back to the early part of the Tertiary period. It will be seen, nevertheless, that it only differs from



RESTORED SKELETON OF HALITHERIUM SCHINZI,
An Extinct Sirenian from the Lower Miocene of Hesse-Darmstadt
(One-fifteenth natural size)

such a Sirenian as the existing manatee in but the smallest particulars. Some of its teeth seem to have been replaced by vertical successors. The vertebrae exhibit distinct traces of terminal epiphyses in young animals. The hind limb is represented not only by the rudimentary pelvis, but also by a trace of the femur. Otherwise, its skeletal parts are almost identical with those of its surviving relative. Since the Sirenians came into existence, indeed, very little change has taken place among them.

The Beaver in Norway

The beaver, which was once so common throughout northern Europe, still survives in Norway, and several notices of its occurrence in that country have been published in recent years. These notices, however, are more or less limited in their scope, and Prof. R. Collett, the eminent zoologist of Christiania, has lately done good service in investigating the whole subject with thoroughness. His results are published, with twelve beautiful photographs of the modern beaver-haunts, in the first article of the *Bergens Museums Aarbog* for 1897.

Trade in beaver skins was carried on in Norway in the Middle Ages, and the former wide distribution of the animal in the country is indicated by reference to it in many place-names. Now, however, its range is much restricted, and unless the laws for its preservation are rigidly enforced it will soon become quite extinct. It is chiefly confined to the Stifts of Christiania and Christiansand, and the largest colony is at present located in the middle and southern parts of the river Nisser (or Nid) in Nedenaes Amt. The banks of this river are for the most part covered with forests of *Pinus sylvestris*, and wherever these are interrupted by trees with deciduous leaves the beaver is to be found. Its chief food is the fresh bark of the last-mentioned trees, especially of *Populus tremula*; and for winter use small branches with the bark on are submerged in the water in front of the habitations. Bark that has been gnawed off is not collected for winter provender. Most trees are felled quite close to the water, and they are rarely brought from a distance of more than 300 metres.

Immediately after the break-up of the ice in spring, the beaver commences to search for food, and traces of it are sometimes seen in the snow. Work on the lodge or habitation is mainly done in the autumn, and almost exclusively at night. When the animal is seen in the daytime, it is as a rule only swimming in the water without any set task on hand.

The trees cut down by the beaver do not fall in any one definite direction, but lie pointing in every way. The current of the river is used for transport purposes whenever possible; but most of the

lodges are situated in still water, and there the animal must itself convey the logs, holding them between its fore paws, while swimming solely by the hind legs. The construction of a lodge occupies at least two years, and it is repaired annually. It is usually elongated in shape, rarely round or conical, and it is always far distant from its nearest neighbour. Numerous burrows are made in the bank of the river near the lodge, but rarely in connection with it. They seem to be inhabited chiefly by young individuals, and they are the first refuges formed by the beaver at any spot where it has decided to settle down and build.

As to the inhabitants of a lodge, Prof. Collett thinks that only one pair with their latest young occupy each. The older litters either migrate or occupy the neighbouring burrows.

The Fishes of the North Atlantic

We have also been favoured by Prof. Collett with a copy of his handsome memoir on the fishes collected by the Prince of Monaco on his yacht the *Hirondelle* during the years 1885-1888, which is one of the most important contributions to Ichthyology of the last decade.* It is published in the sumptuous style with which the generosity of the Prince of Monaco has now made us familiar, and the illustrations are among the most exquisite figures of fishes we have yet seen.

The fishes obtained belong to ninety-five species, and are of great interest not only in their elucidation of the ordinary pelagic fauna of the North Atlantic, but also in the light they shed upon the geographical and bathymetrical distribution of a certain number of remarkable forms obtained from depths of no less than 2000 metres. Only six new species are determined, and only one new generic name is proposed, namely, *Halosauropsis* for *Halosaurus macrochir* of Günther—an emendation also made some time ago by Goode and Bean, who, however, proposed to term the genus *Aldrovandia*.

The classification of Günther is mainly adopted in this memoir, and the new specimens of the more important species are carefully described in detail. The value of the work is also greatly enhanced by the copious references to the literature of the subject, and the comparison of the results with those of previous authors. It would tend much towards the progress of systematic zoology if this laborious method were more generally followed by the authors of such reports. When recording new facts, it adds much to the toil of the work to incorporate them and correlate them precisely with existing knowledge; but the additional labour is well spent, and it converts dry catalogues, comprehensible only to a few narrow

* Poissons provenant des Campagnes du Yacht *l'Hirondelle* (1885-1888). By Robert Collett. 4to, pp. viii., 198, pl. vi. (Résultats des Campagnes Scientifiques accomplies sur son Yacht, par Albert 1er, Prince Souverain de Monaco, fasc. x., 1896.)

specialists, into readable narratives which any zoologist can appreciate.

The Study of Botany

At the meeting of the London County Council on October 12, the following resolution was adopted:—"That it be referred to the Parks Committee and to the Technical Education Board, to consider and report as to the practicability of laying out plots of ground in certain parks in such manner as will afford assistance to scholars of elementary and secondary schools in the study of practical botany." We hope the Committee and Board concerned will report favourably and that some of the resources of the London parks will be turned to account in the interest of the humble student of botany. If we consider only those who sit each year for the examination of the Science and Art Department, there must be a large number of students scattered through the metropolis, to whom the suggested arrangement would be very welcome. Examiners tell us that answers to the questions show knowledge derived mainly or entirely from books, and insist on the necessity of more thorough practical work. But the London student has not much opportunity for such. The Botanical Department of the British Museum in Cromwell Road, by means of carefully dried specimens, models and illustrations, supplies an excellent systematic review of the plant-world, and by skilfully prepared fruits and seeds, and wax models remarkable for their life-like accuracy and beauty, associated with clearly written labels and explanatory sketches, demonstrates to all who come to see such matters as the structure and mode of operation of insect-eating plants, or the means of distribution of fruits and seeds. But there is still much which can only be learnt from the living plant. We believe that Kew and the Royal Botanic Society's Gardens are the only ones to which the student can get access; the former by right (after 12 o'clock), the latter by courtesy only of the Council at certain times on certain days. The latter are useful for those living in a certain part of North London, while a journey to Kew means the underground railway or a happy day on the South-Western. And after all, life is short, and there are often other subjects which must be studied in addition to Botany. With even very little alteration or additional expense a park, such, for instance, as Battersea Park, might be made very helpful to an elementary student. There is a sheltered path by the lake where, in the summer, tree-ferns and cycads flourish, and in the same sub-tropical garden grow palm trees, most of them quite large enough to show a characteristic habit. But if we remember aright, many have no labels at all, and labels when present are very inadequate. A bare binomial name conveys little information; the addition of the group or order to which the plant belongs,

and the country of which it is a native will help to crystallise certain ideas about that group or order which have been vaguely floating in the student's mind. A considerable sum of money must be spent every year in providing the chrysanthemum shows which are to be found in many of the parks each autumn. Some of the chrysanthemums are beautiful, others very ugly; but we are of opinion that the money would be better spent in keeping a variety of plants which need the shelter of a house all the year round. Such as, for instance, a few temperate orchids, examples of insectivorous plants and the like, which the ordinary student knows only from pictures. The resolution now before the County Council has reference probably to laying out beds to illustrate some of the more important families. This might be done at very little expense, and under proper management would be a great boon to the would-be botanist.

Botany of the Azores

The Eighth Annual Report of the Missouri Botanical Garden, issued by Prof. Trelease, the director, has just reached us. It is prepared in the same clear and elaborate style as its predecessors, and gives an exhaustive account of the work, educational as well as horticultural, achieved during the year. Excellent reproductions of photographs taken in the gardens are again an interesting feature. One gives us an idea of the destruction caused by the memorable tornado of May 27, 1896, which worked such havoc in St Louis. Though the grounds were not actually traversed by the cyclonic funnel, the violence of the wind was such that a number of the structures were either unroofed or totally wrecked, while some 450 trees, often of large size, where wholly or practically destroyed, and many of those left standing were seriously broken. Six days before the tornado "the most destructive hailstorm that has ever been experienced at the garden," also caused great damage. The scientific papers which occupy the greater part of the volume are extra-American in interest, and embody the results of some work by the director in the Azores. Mr J. Cardot supplies an account of the mosses found on the nine islands. These (excluding bog-mosses, of which there are eight) number eighty, fifty of which occur also in Madeira and the Canaries, sixty-one in Europe, especially the Mediterranean region, and in Algeria, and about forty in North America. One found in Flores has been known hitherto only from Madagascar and equally distant relationships are shown in the distribution of the genus *Sciaromium*, which has three species in the Azores, while nearly all its other representatives are to be found in New Zealand and temperate South America. Seven species are described for the first time. The fact that several of the most important islands do not as yet muster together more

than fourteen species indicates that there is still much to be done before a satisfactory knowledge of the bryology of the islands is attained, and Mr Cardot believes that the number (eighty) could easily be doubled. The fungi are practically unstudied, and many species should be found in the moist wooded regions. The list of marine algae, "though unquestionably small, may doubtless be increased considerably by collections prosecuted through the entire year, while there is reason to expect a very large number of diatoms and desmids, as well as many representatives of other groups of fresh-water algae, whenever careful collections shall have been made." As regards flowering plants and ferns, though the list is probably nearly complete, there is still scope for much interesting work, such as a detailed local flora for the islands, with an analysis of the influences which favour the extended distribution of one species while restricting another to a very limited area. For such an enterprise the catalogue which Prof. Trelease appends to his botanical observations would form a useful basis. Most of the species, it is suggested, "may have been introduced by ordinary means, largely through human agency, since the discovery of the islands, for they are so precisely comparable with similarly named species from other parts of the world as to suggest the lapse of a very short time since their separation from the parent stock." Only a few are peculiar. Some of the latter are limited to one or other or several of the islands; but the native flora has clearly suffered so much through the inroads of man and domesticated animals, that it is impossible to say whether or not these local limitations have always existed. The greater number of the flowering plants are either wind-fertilised or adapted for pollination by but little-specialised insects, having as a rule open flowers, with readily accessible nectar or pollen. As regards relation between plants and animals, Prof. Trelease remarks that, as there are only seven species of wild mammals found in the islands, and nine endemic or commonly concerned with plant-dissemination elsewhere, and few birds capable of aiding in this work, except for aquatics or marsh plants, "it is scarcely to be expected that special dissemination adaptations would be found on the part of aboriginal plants, which presumably have been associated with these animals for a relatively short time, nor of recently introduced plants, unless the relations have been established and the modifications worked out before either plant or animal reached the Azores." Well developed burrs, for instance, are found only on recent introductions, and the great majority of species "either have no special modification adapting them to certain dissemination, but depend upon gravitation, the wind, or hygroscopic movements of their seed vessels, or else their adaptations are out of harmony with their surroundings."

I

The Fundamental Principles of Heredity

(*Concluded from p.* 239)

THE power of propagation of animals by small fragments is possessed very largely by Sponges, some Coelenterates, Starfishes and certain Flat worms; it is practically lost in the higher groups for several reasons, considerations of nutrition being most important. An Animal fragment can only obtain the nutritive matter for forming new cells by eating up, as it were, part of itself, until it has formed new organs for the prehension and digestion of food. To do this, the fragment must be always big enough to render this sacrifice possible; and, moreover, the tissue-cells must not be too specialised to adapt themselves to the altered conditions. Thus, the complex tissues of a human arm, accustomed to be served by a constant supply of blood current bearing in an abundance of food and oxygen and carrying off all waste materials, and to the guidance of a highly developed nervous system, can never adapt themselves to a life of isolation. In this respect Animals contrast markedly with Plants.

To study in the way we have applied to Animals the laws of reproduction and propagation in Plants, we must revert to those Protists whose life is essentially vegetal. These possess a coloured portion of protoplasm (green, yellow, or red), in which, under the stimulus of light, inorganic materials are combined to form the organic food on which (like animals) they feed. As these inorganic materials exist in solution, they can soak into the cell, which needs neither mouth nor stomach; and the cell can exist, grow, and multiply by division at the limit of growth, even while invested with a thin coating of the papery material, cellulose. If the cell start as a cylinder or ovoid, and the divisions are always in the same direction, at right angles to its length, the product (a colony of our first type) is an elongated filament, like those which form the green, slimy scum on our way-side ditches; if the divisions take place in two planes, the colony will form a plate or disk; if in three, a solid mass which is much more rare. When a period of increased vital activity ensues, brood-formation sets in; the brood-cells are at first naked, lacking the cellulose wall, and usually provided with swimming lashes. The brood-cells may in one and the same species[1]

[1] The filamentous Alga *Ulothrix zonata*.

have very different fates. They may (1) settle down within the wall of the parent cell, and grow out into filaments, which finally rupture the parent cell-wall by their elongation; or (2) they may escape, and only after swimming about for a short time settle down to grow into filaments; or (3) they may pair first of all, and then

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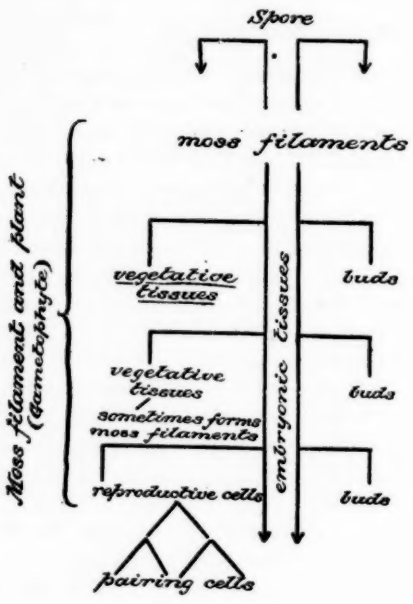

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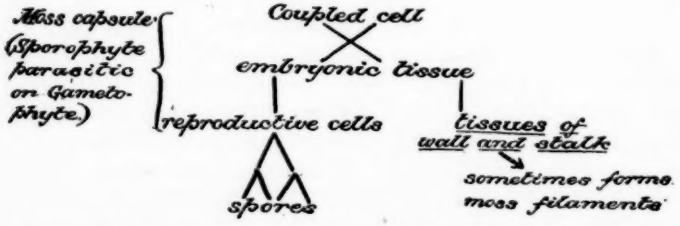

the coupled-cell, after a rest, makes a fresh start of life and growth and multiplication within the cell-wall. The life-cycle may be very complex. We may even find states in which the cell-walls of the filament gelatinise, and the cells themselves round off, the colony forming a very irregular mass.

In some forms that are in other respects very primitive, we find a true differentiation that has advanced further than *Protospongia*, the lowest animal type we have selected as an illustration. *Volvox globator* is a beautiful green sphere the size of a small pin's head, found actively rolling over and over, as its name implies, in still waters fully exposed to the light. On microscopic examination it is seen to consist of many hundreds or even thousands of green cells imbedded in the surface of a spherical mass of gelatinous cellulose, and sending their active lashes into the water. Scattered among these are a few larger cells, which may be seen in all stages of segmentation; and as these grow and segment, they protrude into the cavity of the sphere, and finally rupture it and become free as new individuals. The ruptured sphere sinks to the bottom, and the colonial cells at its surface soon die, whether from the unfavourable conditions or no it is impossible to say. At the time for pairing it is only the few large cells that become or give birth to pairing-cells; the resulting coupled-cell segments to form a new colony. Here again we have a well-marked sterilisation of tissue-cells, and their characters are transmitted only through the reproductive cells, their collaterals. From our standpoint *Volvox* must rank as a lowly Metaphyte.

The majority of Metaphytes show a much higher differentiation and a power of colonial propagation far greater and more continuously exercised than in any Animals.

The first that we shall consider are the Scale and Leaf-mosses. As is well known, the little capsule or urn is full of a fine dust consisting of reproductive brood cells or 'spores.' These germinate and grow, as in Protophytes, into filaments consisting of elongated cells, some of which are green and run on the surface of the ground, while others penetrate it and serve as roots. But so little specialised are they that the reversal of a minute sod containing them will determine a change of their relative character and functions. On branches of these other cells are formed, which are short and thick. These divide, and by their colonial growth the proper leafy moss-plant is formed, but only the lower part for the time being assumes the condition of the moss tissues, the uppermost cells being colourless, nourished by the green cells of the stem and leaves, and assuming and retaining the functions of an embryonic tissue. This constitutes the 'growing point' characteristic of all the higher plants.

Ultimately, in the deeper parts of certain outgrowths, near the growing point, are formed reproductive-cells which give rise to pairing-cells, male or female, as the case may be. Fertilisation is internal, the male cell swimming up to the immovable female, and fusing with it in situ. The coupled-cell remains imbedded in the Moss-plant, and is nourished thereby as a parasite, and, undergoing segmentation, is converted into a colonial mass. The

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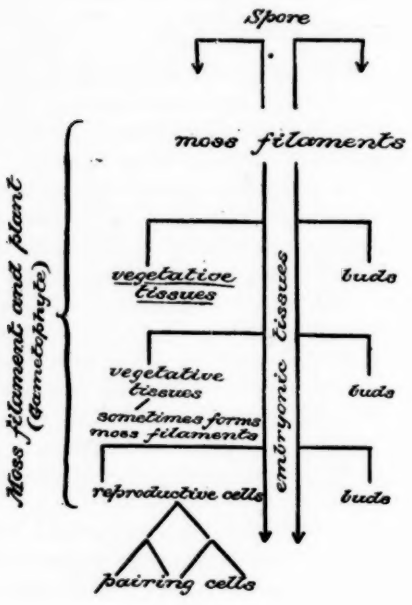

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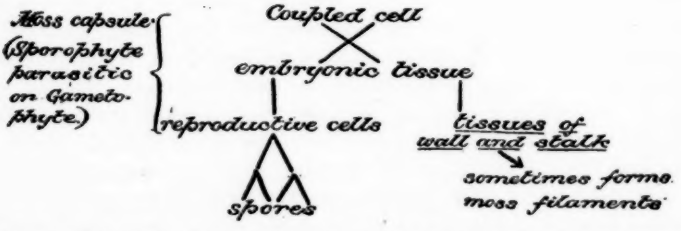

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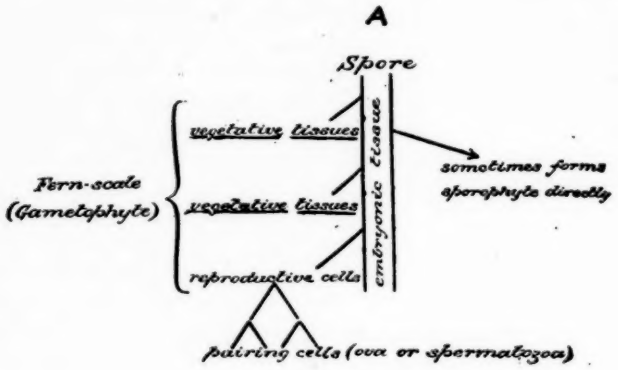
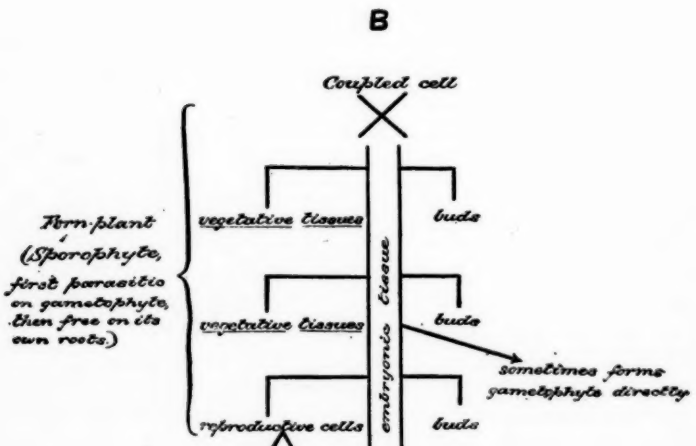
outer layer of this colony in the most primitive Scale-mosses is converted into a capsular wall, while the inner cells are reproductive cells, each of which forms a brood of four spores. In the Leaf-mosses the colonial body formed by the segmentation of the coupled-cell is much more modified, with increase of specialisation





and corresponding sterilisation; for its lower part is converted into a bristle-like stalk, and the wall and centre of the urn-shaped capsule are both composed of green tissue adapted for the formation of organic food materials.

Before we group these facts into a table we must notice the

extraordinary powers of propagation of the Moss-plant: if cut up into fragments, almost any green cell, whether of the Moss-plant or the young urn, is capable of growing out into a green filament that will produce new leafy plants; and this is in addition to the propagative power by ordinary branching or budding of the embryonic tissues at the growing point. We will, according to custom, begin our table with the coupled-cell.

It will be seen here that there is no necessary colonial death (as in *Volvox*) of the leafy Moss-plant, though the older tissues of the stem and the leaves usually die down after the maturation of the parasitic capsule, and that the power of propagation possessed under certain circumstances by the green cells of the Moss-plant and urn make them possible direct ancestors of reproductive cells.

Still, in what we may regard as the normal cycle, the reproductive cells produce among their offspring collaterals as well as direct ancestral forms. The character of the cycle is noteworthy; two systems of colonial growth each beginning with a single cell are determined or closed by the production of brood mother-cells; and these systems contrast both in the characters of the colony and in the nature of the brood-cells. The colonial outcome of the spores is the filamentous growth and the leafy Moss-plant, and the brood-cells formed therefrom are the sexual pairing-cells; the colonial outcome of the coupled-cell is the capsule, and its brood-cells are the asexual spores. This is then an 'alternation of generations' in the sense of colonial or habitual terminology. Botanists have termed the contrasting colonial plants 'Sexual' and 'Asexual,' Gametophyte and Sporophyte, respectively from the character of the brood-cells which each produces in turn.

In the ascending scale of the Vegetable Kingdom we first meet in the Moss-plant with those tissue-cells which we term 'embryonic'; these must be defined as colonial cells nourished by the adult part of the colony, and having for their sole function growth or continued division at the limit of growth to form new cells and organs. Such cells are obviously not at all 'primitive,' as they are frequently called, but on the contrary are the essential outcome of high colonial differentiation. That the whole colony may exist in this condition in the early stages of development is only rendered possible in the case of the Moss-urn by its receiving nourishment as a parasite from the leafy plant.

The Fern is only comparable with the Moss by a complete detachment from preconceived ideas. Most readers know that the Fern sheds from the brown ridges or spots on the under side of its leaves a fine dust, whose particles are the spores. Each spore in germinating produces a cellular filament, which soon expands into a green plate, the equivalent of the leafy Moss-

plant, or, better, the 'plant' of the Scale-moss or Liverwort, to which it bears a close resemblance. On this are borne sexual organs which produce sexual cells. The coupled-cell, as in the Moss, is at first parasitic on the scale, and develops into a Fern 'plant,' such as we know it with stem, roots, and leaves, and finally spores. The essential difference here is that in the Mosses the spore-forming plant is entirely parasitic and of limited growth, while in the Fern it becomes independent, and is of unlimited growth, being provided with organs of support and conduction as well as of nutrition. We may well say that the sterilisation (to use Bower's term) of part of the colony has led to so extended a power of colonial growth and branching, that the power of forming reproductive cells is in the end enormously increased. The propagative capacities of Ferns by buds from embryonic tissue are very great; those of fragments of the spore-bearing plant are slight; but the sex-bearing scale may be artificially propagated by being cut into small pieces, although its life is usually limited by the formation of the parasitic Fern-plant from the coupled-cell.

Ferns then show the same alternation between spore-bearing and sex-bearing generations as Mosses, but the order of relative conspicuousness and abundance of colonial growth is inverted. We have seen that in Mosses a vegetative transition by cell growth might take place from the spore-bearing generation to the other. In Ferns similar transitions are possible both ways, so as to cut out the stage of brood cell formation, which we regard as the critical reproductive stage.[1] Thus in many Film-ferns, instead of producing spores, the leaves grow out into scale-plates bearing sexual organs, while in the common Cretan fern, the scale produced from the spore grows out directly into the spore-bearing leafy Fern-plant instead of giving rise to sexual cells. In flowering plants the relations of the sex-bearing plant are much obscured, and it would lead us too far to explain them here. Suffice it to say that the 'plant' as we know it corresponds to the Fern-plant or moss-capsule: it is the Sporophyte, not the Gametophyte. The parasitism of the embryo formed from the coupled-cell is usually intense and prolonged.

A very remarkable character of Dicotyledons or Exogens is the continuation downwards from the growing point of a zone of embryonic tissue, the 'cambium,' which habitually by its growth and multiplication forms zones of wood on the inner side, and inner-bark (or bast) on the outer. This layer has, in cuttings, an especial tendency to form buds. But all the living cells retain a power of forming a similar tissue at or near an exposed surface; for instance, such a layer is formed a little within the surface of trees to produce the

[1] These transitions have been aptly termed "short circuitings" by Sir Edward Fry.

cork,—this is known as the cork-cambium. We are all of us familiar with the little brown scars on plums, &c., that have been slightly injured when green: these are due to the local development of a layer of embryonic tissue below the injured surface, and the formation of a thin protective layer of cork therefrom.

Colonial propagation in Flowering Plants may take place by the separation of buds (which form normally at the growing point), or by development of so-called adventitious buds from the embryonic cambium zone of the stem or roots. Such propagation by minute fragments as occurs in Mosses is unknown here; but larger fragments of leaves can frequently produce buds and ultimately plants. The cells within the cut surfaces produce an embryonic tissue, which gives rise both to a protective skin of cork and to adventitious buds.

The readiness to form cork and adventitious buds in this way varies extremely, and with this the power of leaf propagation. For the formation of cork is an indispensable protection against the opposite dangers of drying up on the one hand, and of the attacks of microbes and moulds on the other. Again most Begonias are readily propagated by pieces of leaf; but the bulbous varieties form a mass of embryonic tissue, well protected by cork, which remains for months or years before active buds are developed, so that they were long thought incapable of this mode of reproduction. Not only Begonias, but Gloxinias and other members of the showy order Gesneriaceae, the Peperomias with their massive speckled or veined foliage, and Chrysanthemums, are habitually multiplied in this way; and the list of possibilities in this direction is daily increasing.

On reviewing these facts we see that the law of collateral transmission applies to Plants as well as to Animals, but that they have much greater powers of colonial propagation, by the formation of embryonic tissue from already specialised colonial cells, and by the persistence of a portion of the colony (the growing point, and in Exogens the cambium layers) in the embryonic state. The fact that green cells can manufacture plant food in the light explains the greater vitality and propagative power of small Vegetable fragments as compared with those of Animals; and it is needless to assume any more recondite intrinsic differences. Even in this mode of propagation, the law of collateral transmission holds; for many of the cell-forms of plants, such as hairs, wood-cells, &c., are absolutely sterile, and consequently can never take part in the formation of an embryonic tissue capable of giving rise to a new plant.

Thus, throughout the Higher Kingdoms we find the problem of heredity rests on different data to those supplied by the Protista. In these lowly forms, where the law of direct transmission prevails, it is easy to admit that when a cell resolves itself into two new ones which exactly reproduce its original state, they should each possess

its original qualities; even where the transmission is alternate, we may admit that the different conditions at the different stages of a genetic cycle modify the organisms produced. In the simplest case of collateral transmission, as presented by *Volvox globator*, the sterilised colonial cells so closely resemble more primitive independent forms in their behaviour and character, that we may well believe that they have inherited these from such forms, directly and unaltered, from some Protist ancestor, while the reproductive cells have become modified. But it is impossible to suggest such an explanation for the higher Animals and Plants, since a nerve-cell with its outgrowths many feet long, or a woody fibre which has expended all its living protoplasm in the building up of a firm wall, can only have been evolved as portions of a highly-specialised colonial organism.

The difficulty of explaining the mechanism of collateral transmission in Metazoa and Metaphytes by the direct transmission in Protista has been the origin of the recent lively discussions on heredity. To biologists saturated with the implicit conviction that only direct cellular transmission was alone possible, some mysterious agency, that should be contained in the reproductive cells, and be handed down by them in their direct cellular descent, was an essential assumption; and this agency is supplied by Weismann in his Germ-Plasm Theory. The reader will do well to bear in mind that it has been presented to the world in successive editions; each has been greeted as final by the disciples, who have made light of the objections raised thereto, though on every occasion such objections induced the Master to recast the theory in his next work. Our presentment of the theory upheld in the "Germ Plasm; A Theory of Heredity," published in London in 1893, may therefore, for aught anyone can tell, become obsolete very shortly, owing to the author's "having (to use his own phrase) in the meantime gained a deeper insight."

Weismann conceives that in the nucleus of what we have termed 'reproductive' (and also, in part, 'embryonic') cells is a mixed plasm, the 'germ-plasm,' composed of certain entities, the 'determinants' for the several organs of the colony; that when the cell divides at the limit of growth into two similar cells, the germ-plasm and the several determinants divide in the same way, so that the determinants are the same in each of the daughter-cells as they were in the parent. But in those divisions which give rise to specialised cells the germ-plasm divides as a whole, in such a way that the determinants are only distributed between the daughter-cells, some to one, some to another: we may say that there is distribution or repartition, not the true division of the several determinants. Similarly, the determinants each contain a group of minor entities the biophors, and in the ultimate divisions of the cells

of an organ these biophors are distributed between the cells; and the proper biophors in each cell constrain it to play its specific part in the organism.

Those cells which constitute the direct line of descent between the reproductive cells of one generation and those of another are formed by true divisions of the germ-plasm, with all its determinants. But we are met by the facts of propagation by fragments composed only of tissue-cells in Animal, and still more in Plants, where specialised tissue-cells revert to an embryonic condition, or rather beget embryonic cells with a complete germ-plasm. To explain this difficulty, we must suppose that in these cases a portion of complete germ-plasm has passed at their formation into such tissue-cells, and that it has remained dormant until the stimulus of separation from the colonial organism has revived its vitality. Again in the four-celled stage of the segmented embryo of various widely distinct Animals (even in the sixteen-celled stage of some Medusae) it is possible to isolate a single cell, which then develops into a complete embryo, though had it remained associated with its fellows it would have formed only a definite part of the embryo. Here again we find the assumption of the existence of dormant determinants, which become active only in the separated cell, adduced by Weismann to save the theory. This assumption is also used to explain alternation of generations, where the Moss-plant and Moss-urn, or the Fern-scale and Fern-plant alternate; their germ-plasm must contain two sets of determinants, one for the first, the other for the second generation, alternating in sleep and waking like the printer and the hatter in Box and Cox. We are reminded of the complex epicycles required to render the universe workable on Ptolemy's geocentric hypothesis, and the Spanish king's comment thereon. "Had I been consulted at the creation, I could have simplified matters."

So far indeed, this might be held as a formal or fictive hypothesis to explain the mechanism of heredity on the basis of Special Creation—each organism being created fully equipped with its own proper germ-plasm, determinants, biophors, and all. But no; Weismann is a firm believer in the theory of common descent, and, as we have seen, he and his school profess to be the only true Darwinians; and we come to his Theory of Variations.[1]

The germ-plasm with its contained determinants, as it lies in the reproductive cells of the body, is subject to nutritive changes, and consequently to constant slight variations which apparently are not correlated with anything else whatever. The haphazard variations, of the determinants induce corresponding, and

[1] This essay was written nearly two years ago. Since then Weismann has enlarged his theory by the hypothesis of germinal selection. Without going into this we may note that it makes no difference to the present argument.

therefore haphazard, variations of the organism; and the Almighty Natural Selection now steps in, weeds out the unfittest, and so induces the endless variety of form and function in the Organic Realm. This has been irreverently termed the 'toss-up' or 'dice-box' theory of variation. It is hard to see how variations in feeding or starving hypothetical determinants can have ever ended in the development of a vertebrate eye, or in the exquisitely co-operating organs that render possible the parasitism of the offspring on the viviparous mother: it would be difficult if we had limitless aeons of biological time at our disposal, instead of the paltry million of centuries conceded as an outside limit by Lord Kelvin, even when multiplied by 4000, as Perry and Poulton suggest. We have all heard of the German astronomer who was reading Lucretius, and said to himself as noontide approached, "So if the atoms had been flying about for all time, cold potato, oil, vinegar, garlic, and salt might have combined to form a salad." "Yes, dear," said his wife, who had come in unperceived to call him to dinner, "but not as good as you shall have with your beef."

It must be admitted that marvellous ingenuity is shown in giving explanations on this theory to cases where they are not needed; we may cite the limitations of propagation by small fragments of Animals or Plants, and the variations in the power of leaf propagation in the latter, which are so readily explicable without the germ-plasm hypothesis. On this hypothesis, however, we are asked to overlook the plain and obvious questions of nutrition, cork-formation, and bud-formation, and to concentrate our ideas on the presence of more or less dormant germ-plasm in the tissue-cells. We may well note here that among "Inductive Fallacies" Bain cites the error of assigning more causes than a phenomenon needs. "It is involved in the very idea of cause that the effect is in exact accordance with the cause; hence the proof that more causes were operative than the effect needs defeats itself."[1]

But the cardinal defect in the theory is its objective baselessness. It professes to be founded on the microscopic study of the changes in the nucleus in cell-division; but there we find nothing to justify the assumption of two modes of nuclear division in the embryo, the one dividing the determinants, and the other only distributing them between the daughter-cells. To justify such a theory there should at least be some such basis in fact, as indeed there is for the author's 'id' theory of the relations of 'amphigonic' inheritance (from two parents),[2] which does not

[1] "Logic," by Alexander Bain. Part II., Induction, ed. 2, 1873, p. 395.

[2] To avoid complication and the undue lengthening of this essay we have been obliged to omit the consideration of the effect of double parentage in the higher organisms that reproduce sexually. But it is obvious that of itself it must tend to efface and not to accentuate the variations from the average standard of the race.

come within the purview of the present article. As it is, the theory falls under the ever-trenchant blade of Occam's razor, "Entia non sunt multiplicanda praeter necessitatem."

The antagonistic school, of Herbert Spencer, regard Living Beings as characterised by their continuous adjustment of internal relations to external conditions, and cannot see à priori grounds for regarding the reproductive cells as especially lacking in this power of adaptation. They regard instinct as only explicable as habit transmitted and relatively fixed by constant transmission from one generation to the next, and are disinclined to admit (even as a formal hypothesis) any scheme that leaves all such considerations on one side. They therefore are compelled to refer variations in the offspring to the adaptive reaction of the parent to the environment, and hold that there must be some mechanism of transmission other than that of direct cellular inheritance, by which the reproductive cells hand down to their differentiated cell-offspring the characters of the corresponding cells in the parent organism as a whole.

Charles Darwin felt this need so keenly (in a way largely ignored by those who style themselves his only true disciples) that he formulated his elaborate provisional hypothesis of Pangenesis to supply the mechanism that he postulated. He supposed that every cell in the body gave forth minute buds or 'gemmules' which circulated in the blood, and were carried by its current to the reproductive cells where they were stored up, and that in the development of the embryo they induced the formation of cells like those from which they were given off. Galton tried the crucial experiment of transfusing blood from one breed of rabbits to another, and found that this had no effect on the purity of the offspring. This not only shattered the theory of Pangenesis, but settled in the negative every conceivable theory of hereditary transmission based on the conveyance of formed material particles or of chemical substances from the other parts of the colonial organism to the reproductive cells.[1]

The second theory is that of Herbert Spencer, of 'biological units,' of definite form and relation, which by their polarity tend to complete the organism. I shall describe that development of it recently put forward with great skill and ingenuity by Wilhelm Haacke under the title of the "Gemmaria theory."[2] He holds that all living plasma is composed of minute units, the 'gemmae,' grouped together in aggregates, the 'gemmaria,' both being of definite form and size, in virtue of which they tend to assume certain relations of

[1] "Life and Habit," Lond. 1878; and "Unconscious Memory" (Lond., 1890). The latter work contains a translation of Hering's paper. "A Theory of Development and Heredity," by Henry B. Orr (London and New York, 1893), is written essentially from this point of view.

[2] See "Gestaltung und Vererbung," Leipzig, 1893, and "Schöpfung der Tierwelt." Both these works are written in a German style of exceptional charm, ease, and vivacity.

equilibrium in the cells and in the whole organism. Owing to this being a labile equilibrium, any disturbance due to an altered condition of the environment will alter the 'set' of the gemmaria and change the conditions of their equilibrium. It is as the result of their relation to the organism at large that the gemmaria of the reproductive cells R of an organism A are compelled to reproduce the likeness of A; consequently when the continuance of altered surroundings alters A to A′, the gemmaria of the reproductive cells will get a 'set' changing them to R′, which will reproduce the altered organism A′. Now, as a formal hypothesis, this serves to give a very pretty provisional explanation of many phenomena of organic life; but we have no sufficient microscopic evidence in its favour, and, to me at least, much that speaks against it. We know too little of the physical relations of cell-life to be able to accept, even provisionally, a theory based mainly on geometrical and mechanical conceptions.

The most satisfactory explanation, perhaps, is that put forward by Hering and Samuel Butler,[1] the latter of whom has written with singular freshness and an ingenuity which compensates for the author's avowed lack of biological knowledge. This theory has indeed a tentative character, and lacks symmetrical completeness, but is the more welcome as not aiming at the impossible. A whole series of phenomena in organic beings are correlated under the term of memory, conscious and unconscious, patent and latent. Our memory is conscious, when we say a lesson or remember a birthday; unconscious, when we let our fingers play of themselves a piece of music of which we could not write down a note; patent, when we remember to call at a friend's house; latent, during the interval while the servant is waiting at the open door, until the sight of the familiar stick in the hall recalls the owner's name which would not recur to our consciousness. Of the order of unconscious memory, latent till the arrival of the appropriate stimulus, is all the co-operative growth and work of the organism, including its development from the reproductive cells. Concerning the modus operandi we know nothing: the phenomenon may be due, as Hering suggests, to molecular vibrations, which must be at least as distinct from ordinary physical disturbances as Röntgen's rays are from ordinary light, or it may be correlated, as we ourselves are inclined to think, with complex chemical changes in an intricate but orderly succession. For the present at least the problem of heredity can only be elucidated by the light of mental and not material processes.

QUEEN'S COLLEGE, CORK. MARCUS HARTOG.

[1] "On Memory as a Universal Function of Organised Matter" (Vienna, 1870, ex. S. Butler in "Unconscious Memory," p. 97).

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II

Reproductive Divergence: A Factor in Evolution?

IN the September number of *Natural Science* (p. 181) Mr H. M. Vernon propounds a new theory, called Reproductive Divergence, which, he says, is essentially different from Romanes' Physiological Selection. Inasmuch as both Reproductive Divergence and Physiological Selection are a process, not a cause, are based on the occurrence of the same kind of variation among the individuals of a species, and have the same end in view, the differences between the two principles do not appear to me to be of great importance; the premise of Reproductive Divergence is, however, more general, and the way in which the principle is demonstrated is certainly independent. Reproductive Divergence (like Physiological Selection) is brought forward to show that under certain propositions given differences between the individuals of a species inhabiting the same locality and presumed to stand under the same external influences will develop into specific differences solely by means of Reproductive Divergence. What we have to understand by specific differences is quite clear in this case: it is that kind of difference which we find to exist between two morphologically very closely allied forms which, though existing together in the same locality, are entirely independent of one another, the two forms (1) breeding true, the one never producing an individual that belongs to the other, and (2) never fusing into one form, in spite of their not being mutually absolutely sterile. In another place[1] I have referred at some length to Physiological Selection, and endeavoured to show that this principle does not hold good in so far as the outcome of Physiological selection, as propounded by Romanes, is, at best, dimorphism, not specific distinctness; and I came to the conclusion that forms deviating from each other in the way just mentioned cannot be evolved without the aid of some kind of local separation.[2] It does not seem to me that Mr Vernon's arguments in support of Reproductive Divergence as a factor in the evolution of specific distinctness are any more valid than those which were adduced by Romanes in favour of Physiological Selection. The occurrence of such a

[1] "Novitates Zoologicae," 1896, p. 426 ff.

[2] Local races are now generally termed subspecies; their high significance, especially in questions of general Biology, will doubtless be recognised in time also by those systematists who still persist in ignoring subspecies.

correlation between morphological characters and fertility of the specimens of a species as the theory requires cannot be denied. It is quite conceivable that, for instance, in insects the copulatory organs of one or the other species vary correlatively with the size of the individuals in a similar way as the horns on the head and thorax of Dynastid beetles, or the mandibles of stag-beetles do; such a variation in the copulatory organs of the males and a corresponding variation in the females would necessarily have the result, that copulation between specimens of different size could be less easily effected, and would be less effective than copulation between individuals of the same size. Hence the premise of the theory, or 'the first part,' which 'can only be verified by experiment,' must readily be accepted, and would be admissible even if there were as yet no facts observed which proved that the required variation actually occurs. Mr Vernon promises (p. 185) to make further experiments in this direction, for which biologists will surely be very thankful.

The second part of the theory, or the statement, that, if the above premise is given, a species will necessarily develop into two or more varieties or even fresh species, is 'demonstrated mathematically.' Let us examine this mathematical demonstration. Mr Vernon divides the 1800 specimens of a hypothetical species, which is assumed to vary in size from 64 to 73 inches, into three sets of males and females, of 300 specimens each, the sets being designated as S, M, and L, and s, m, and l respectively; then, if it is further assumed, that on an average of the 300 S, 100 S will copulate with s, another 100 with m, and the third 100 with l, and so also in the case of M and L, the total number of offspring will be—the total number of individuals is accepted to be constant in each generation—

I. 100 Ss, 200 Sm, 300 Mm, 200 Ml, 100 Ll.

If now "the comparative fertility of the various sized individuals is slightly changed," so that for instance 100 specimens copulating with individuals of the same size will give birth to 120 offspring, 100 specimens copulating with individuals of slightly different size will produce 95 offspring, and 100 individuals copulating with specimens of considerably different size will give birth to 80 offspring of either sex, the total number of offspring will be distributed as follows:—

II. 120 Ss, 190 Sm, 280 Mm, 190 Ml, 120 Ll.

By a comparison of II. with I. Mr Vernon comes to the two conclusions (*a*) that the limits of variation, which originally were 64 and 73, will be altered to 62·5 and 74·5; and (*b*) that the individuals

of intermediate size will, in succeeding generations, decrease in number, while the individuals of small and large size will increase.

To show that inference (*a*) is correct, Mr Vernon argues as follows:—(1) Variety S varied originally from 64 to 67 inches, the mean being 65·5, and L from 70 to 73, the mean being 71·5; (2) let us then suppose that by the principle of Reproductive Divergence the average of S were reduced to 64, [the specimens varying now from 62·5 to 65·5], and that of L increased to 73, [the individuals varying from 71·5 to 74·5]; (3) then "it follows that these groups S and L would (approximately) contain individuals varying between 62·5 to 65·5 inches, and 71·5 to 74·5 inches respectively." But surely this inference (3) is merely a re-statement of assumption (2)!

And as to the conclusion (*b*) that the intermediate individuals will disappear, it has apparently escaped Mr Vernon that the figures given under II. are nothing else but a re-statement of the proposition that 100 pairs of equal size give birth to 120 offspring (etc.); the result of the chance-breeding is quite different. We must divide the original 900 individuals into five sets, and then compare these five sets with the five sets of II., thus:—

I. 180, 180, 180, 180, 180 = 900.
II. 120, 190, 280, 190, 120 = 900.

It is not for me to point out under which new conditions the range of variation would be widened and the species be split up into varieties. Under those propositions upon which Mr Vernon bases his mathematical demonstration, the mean of S will not decrease, and that of L will not increase, but the smallest and largest specimens will very soon disappear altogether, and the species become monomorphic, as a mathematical consideration of the chance-breeding in succeeding generations will show. If we start with 300 S, 300 M, and 300 L, the number of small, medium-sized, and large individuals in the first generation of offspring will depend on the size of the offspring of each pair; the offspring of a pair may be the same in size as the parents, or may be smaller or larger. It is sufficient to consider two of the infinite possibilities. (1) The 300 S produce on an average equal numbers of small, medium-sized, and large offspring, and so do the 300 M and 300 L. The result will be that the numbers of different-sized individuals will not be altered in succeeding generations, and the variation of the species will also remain the same. This is the usual result of chance-breeding, if no special factors come into play. (2) The 100 S which copulate with 100 s will produce 100 small specimens Ss, no medium-sized and large ones; the same applying as to M and L. This is what Mr Vernon assumes to take place. Though this assumption cannot be allowed to stand, as what is here assumed to be true is one of the characteristics

of specific distinctness which the principle of Reproductive Divergence is propounded to explain, we will accept, for the sake of argument, that parents of the same size breed true. Then of the 120 Ss of the first generation 24 will copulate with small females and 24 each with the other four sets; hence there will be produced only 29 Ss. These 29 will have to copulate with nine sets, and so on. The same applies to Ll. Or to put it generally, if A is the number of individuals of each original set, a the number of original sets, x the surplus fertility, n the number of generations, then under the propositions adduced by Mr Vernon,

$$S_n^n = \left(\frac{100+x}{100}\right)^n \cdot \frac{A}{a\left((a-1)^2+1\right).\left((a-1)^3+1\right)\ldots\left((a-1)^n+1\right)}.$$

In our case the numbers of S in the succeeding generations will, therefore, be—I. = 120; II. = 29; III. = 4; IV. = 0, 3.

That is to say, after the fourth generation, the largest and smallest specimens will be weeded out, and this result will not materially be altered, even if we assume that the largest and smallest individuals are mutually absolutely sterile. (Compare also Galton's regression towards the mean.)

Although Reproductive Divergence does not achieve what Mr Vernon claims for it, it is not altogether to be rejected under other premises than those accepted by Mr Vernon. There are certain species, for instance among Lepidoptera, which vary in the same locality in such a way, that there are two well-marked varieties which breed freely with one another, but produce comparatively few intergraduate specimens, the offspring belonging mostly to the one or to the other variety.[1] Here Reproductive Divergence may eventually have free play, and then necessarily will evolve incipient dimorphism into complete dimorphism, and in so far Reproductive Divergence might be called a factor in evolution.

KARL JORDAN.

ZOOLOGICAL MUSEUM, TRING.

[1] Standfuss, "Handbuch f. Schmetterlingssammler," 1895.—See also Giard, *Natural Science* I. p. 388 (1892); Romanes, *ibid.* p. 398.

III

A New Scheme of Geological Arrangement and Nomenclature

Part I

THE only scientific men whom Charon will carry across the Styx without a fee will probably be those who conform to orthodox shibboleths. For the rest, including most of the editors of and contributors to *Natural Science*, I mean the purveyors of audacious heresy, a heavy charge will no doubt be made. Meanwhile we ought to have our turn in this world and if we shock those who sit on velvet and dislike to have the picturesque dust of their cherished prejudices disturbed, they will remember that they will have their comfort when the Conservative old boatman leaves the flagrantly and impudently wicked in the mud.

In venturing to ventilate a fresh heresy I thought it needed such a preface.

The systematic arrangement of the various beds which compose the Earth's crust began, as is well known, with the Italian writers of the seventeenth century. It was Lehmann, however, who first really proposed a rational arrangement by separating the crystalline unstratified rocks, which he called Primitive, from the beds arranged in successive strata, which he called Secondary.

This classification with modifications including notably the introduction of a third class of beds called Transition, and answering largely to our present Cambrian and Siberian strata, continued in vogue until the beginning of the present century, and it was, in fact, the only possible arrangement so long as petrographical considerations were alone considered of importance in discriminating between different rocks, for it was early and easily seen that beds of very different chemical composition might graduate horizontally into each other, being therefore probably on the same horizon, while in other cases beds of the same chemical composition were clearly situated at different horizons.

The key to the problem which finally unlocked the geological riddle was the discovery, not made at one bound, but first applied systematically by William Smith, namely, that different geological horizons are marked by different species of fossil remains. This prime discovery has, of course, enabled us to map out the long

series of stratified rocks in a continuous chain and to unerringly distinguish any particular horizon in discussion by its particular ear mark, namely, the special fauna and flora whose debris are found in it.

This I need not say is the corner stone of modern stratigraphical geology, and this, so far as we can see it, will remain. I have nothing to say about the key which is an absolutely indispensable one. What I propose to criticise, however, is the arrangement founded upon the facts thus ascertained, and I propose to attack it in two ways and on two grounds. If our object is to ascertain the past sequence of events in any particular spot on the earth's surface, we cannot do better than make a boring at the particular spot and describe in detail the successive beds lying upon one another or which we can fairly conclude once lay upon one another in that particular spot. This will undoubtedly, so far as that spot is concerned, give us the sequence of events. If the record be complete it will, of course, be a complete story. If some pages be torn out of the book it will, of course, be incomplete. This we may or we may not be able to infer. What is clear is that the column of different strata thus pierced represents not a general universal geological pedigree, but the geological pedigree of one particular spot only. This, of course, is universally admitted. No fact is more elementary than that two wells dug in the same parish may present us with very different columns of strata. Some thin out, some grow thicker, some disappear, and some make their appearance. The one cardinal fact, however, remains, that so long as we remain in the same "Zoological Province" so long will these beds when found together be found arranged in the same order.

This being so it is perfectly justifiable and perfectly logical so long as we remain in the same zoological province to collect all the beds occurring within that province and to arrange them in sequence, and having done so to make that sequence a test and touchstone by which the relative position of any particular bed in any particular section may be ascertained; always remembering, however, that we do not mean by this arrangement that the sequence of events in every spot within this area was precisely the same. In some cases certain stages were possibly absent as the record seems to infer, or a particular stage in one area may have become a complicated series in another. These are, however, matters of detail in which we have no necessity to guard ourselves since they are obvious and simple. What it is important to remember, and what has been made the subject of adverse comment by more than one distinguished palaeontologist, is the fact that the arrangement we have been considering only holds good and is alone

useful when applied within one zoological province, and becomes utterly misleading when applied to any other. Let me explain what I mean. Zoologists have divided the land surfaces of the earth into several provinces marked by special faunas. Of these divisions that proposed by Mr Sclater long ago, and which was founded mainly on the distribution of birds, is the most popular. Each one of these provinces is marked by a special animal and vegetable facies. Similar provinces with a similar variation in their inhabitants occur also beneath the sea. Now it is clear that each of these life provinces has a special pedigree of its own. It may be that they all converge eventually upon some common and universal original, but the various lines of descent must have been separate from early geological times. How then is it possible or is it profitable to attempt to measure and test in any way whatever the geological record of one zoological province by that of another? We may eventually be able to say what was the character of the different zoological provinces contemporary with different geological horizons in our own country, but this kind of knowledge will profit us little. What we want to know is the pedigree of each zoological province by itself, and to keep that pedigree intact and separate and unsophisticated by any false correlations with the pedigrees of other provinces. When I am asked if a particular bed in India is Miocene or Pliocene, or a particular bed in New Zealand is Tertiary or Quaternary, etc., etc., I cannot attach any useful meaning to the question. If it mean that the bed is actually contemporary with, or that it is homotaxial with the beds so-called in Europe, the question is desperately hard to answer and of very little use when answered. If it means that a particular bed is the penultimate or the ante-penultimate geological stage in each area irrespective of actual equating of periods and dates, it may convey some meaning but it is a meaning crossed and sophisticated with danger and with doubt. What we want to do if we are to do justice to the great fact of the continuity of life, is to keep the two stories entirely apart, and to do so if possible by using a nomenclature which shall not be misleading.

If then we are to retain the present geological nomenclature and arrangement for the beds of the pan-Arctic or hol-Arctic region where a common fauna now prevails, we ought to apply an entirely different nomenclature to the arrangement of the beds in the Neotropical, the South American, the Indo-African, the Australian, the Indonesian, and the New Zealand provinces. We may then indulge in theories and systems of *homotaxis* without any danger, and we shall always be sure that we are measuring the horizon we are dealing with by a fixed and not by an unstable barometer. This is the first parable I wish to preach.

Secondly. In the last sentence I commenced with a very large "If" because I am afraid my heretical perversity carries me a good deal further than I have yet dared to admit.

Before Herbert Spencer held up his great lamp and bade us see in animal and vegetable life not a discrete collection of disintegrated units but a continuous unbroken chain, it was prudent and wise perhaps to be content with an arrangement of the stratified beds marked by no other law or rule than their mere order of superposition in any particular place. This will no longer content us. We want to know a great deal more than this. We want to know what was the route and road by which a particular fauna and flora came to occupy a particular zoological or botanical province, and what were the stages of its growth and development. For this supreme purpose we must go further afield than merely examine the column of strata existing in any particular place. A very cursory examination at once shows us that in every such column marine beds are intercalated with sub-aerial beds, and no ingenuity can possibly derive the fauna of the land from that of the sea and *vice versâ* by successive jumps and starts. They have absolutely nothing to do with each other, and if our purpose is not merely to calendar the revolutions of land and sea which have occurred in a particular place, but to trace out the history of the particular fauna occupying a particular province, either of the land or of the sea, and thus to track the continuous history of each of these divergent portions of the earth's surface along lines of continuity and growth, we must absolutely discard our present method of geological arrangement and nomenclature for a very different one. In the first place (and the change is so obvious that it has always seemed to me a paradox that it was not made long ago) we must absolutely separate the marine beds from the sub-aerial ones, put them into two entirely different columns and perhaps give them entirely different names. To apply the term Pliocene to the marine beds marked by the Norwich or the Weybourn Crag and to apply it also to the sub-aerial beds known as the Forest bed, is not an illuminating but a darkening process. These two sets of beds may have been contemporary but they have no other element in common, and it is utterly misleading to give them a common name because the marine and land debris are sometimes mixed as in the Norwich Crag just as Ammonites and Mammoths both may be mixed with striated boulders in the soft beds on the coast of Holderness. If we are to retain our present geological nomenclature for the pan-arctic region we must qualify each name by a distinctive epithet showing whether the bed we mean is in the marine or the sub-aerial series.

Again, we continually read in geological books of unconformability, an excellent term expressing a very patent fact when we

are dealing with what I would call parochial geology, but a term which ought to have no place in a general scheme in which the progressive history of a life province is to be illustrated. If there is a hiatus and a gap here, it must be in the evidence and not in the actual story. It is the imperfection of the geological record and not the occurrence of a real unconformability in two successive stages of the history of a life-province which is the infirmity of our inquiry, and we disguise and distort the picture utterly by painting it as we do.

The ideal arrangement of our beds ought to correspond with their history in time as marked, not by their accidental sequence in any particular spot, but by the successive phases of their life contents. The land surface of the Palaearctic zoological province of to-day must have been in geographical contact and continuity with a corresponding land surface yesterday, and so on to the beginning of time.

A succession of land bridges must have connected the present land surfaces with those of the primitive world by a perfectly unbroken chain, unless we postulate the complete periodic destruction of land faunas and their re-creation; and similarly with the marine faunas. This being so, unconformability and break ought to entirely disappear from our series. If we find signs that a marine submergence intervened between two stages of sub-aerial history of some locality and caused a breach between them, we must, nevertheless, conclude that these two stages were connected geographically at some point or points by a third one affording us the intermediate chapter, and it is along these bridges that we ought distinctly to travel. This seems to me to open up an entirely different mode of arranging and studying our beds to that usually current in text-books, one more consonant with modern zoological and palaeontological notions.

Our first step, as I have said, is to entirely separate the submarine and the sub-aerial beds from one another, and to range them in two series. Secondly, to arrange the beds, not according to their vertical distribution in one or more spots, but according to their continuity in regard to conditions of deposition. This will lead us along some unexpected and some not infertile lines of inquiry. Once we grasp this notion we shall cease to attach much, if any, value to the accepted generic terms of stratigraphical geology—the primary, secondary, tertiary beds, etc. In our own country, no doubt, the beds are separated by great gaps, represented fairly by their names, and if we are studying English geology only, the nomenclature and classification are justified; but these gaps cannot have existed everywhere, unless we are to reverse all our modern teaching. The story of biological development must have been quite continuous, and the book in which it was recorded must have contained a continuous

series of pages, unseparated by gaps, dislocations, and unconformabilities, and this in the marine series just as much as in the sub-aerial series.

There are two initial lessons, then, I wish to press home before I venture on to more concrete lessons, namely:—1. That our present system of geological arrangement and nomenclature is only adapted to the purely local sequence of facts and history of the beds of Western Europe, and not to the whole world. 2. That we ought to arrange our beds on a universal and ideal system in two perfectly continuous series, one marine and one sub-aerial, marked not by their superposition in any one spot, but by the successive changes in the biological history of the world, and by no other test.

HENRY H. HOWORTH.

IV

The Authenticity of Plateau Man

THE antiquity of man and the locality of his birthplace are problems of perennial interest, to which Sir John Evans' address to the British Association has again directed general attention. Sir John Evan's emphatic dismissal of all the evidence yet advanced in favour of the existence of man earlier than the date of the Palaeolithic gravels will no doubt arouse controversy, especially as in the time at his disposal he could give only a passing reference to any single case, and not state the grounds of his distrust. As I have during the past year given careful attention to the asserted 'Eolithic' implements found in the high plateau gravels near Sevenoaks, with the result that I have had to abandon my first belief in their human origin, it may be of interest if I state the reasons for my change of opinion.

The implements come from the deposits described by my late friend Sir Joseph Prestwich in an important paper published by the Geological Society in 1891. The chipped flints themselves were described by Prestwich in a paper read to the Anthropological Institute a year later. Further descriptions have been given by their original discoverer, Mr Benjamin Harrison of Ightham, by Professor T. Rupert Jones, Mr Lewis Abbott, and other writers. Some geologists objected to the idea that the plateau flints had been worked by man at the time of their first description; but the drift of expressed opinion has been lately rather in their favour.

Early in 1896 I visited Ightham, and Mr Harrison kindly showed me the great collection of chipped plateau flints which he has formed during the past thirty or forty years with an indefatigable perseverance that has excited the admiration of every student of archaeology. As Mr Harrison's specimens lay side by side there appeared a remarkable recurrence of the same external form. This fact led me to accept the conclusion that the specimens had been shaped by man, but a more searching examination necessitated the abandonment of this opinion, as the flints themselves tell me quite a different story.

Let us first examine the flints and see what traces they show of the natural agencies that have acted upon them.

The flints occur as a gravel on the surface of the chalk plateau,

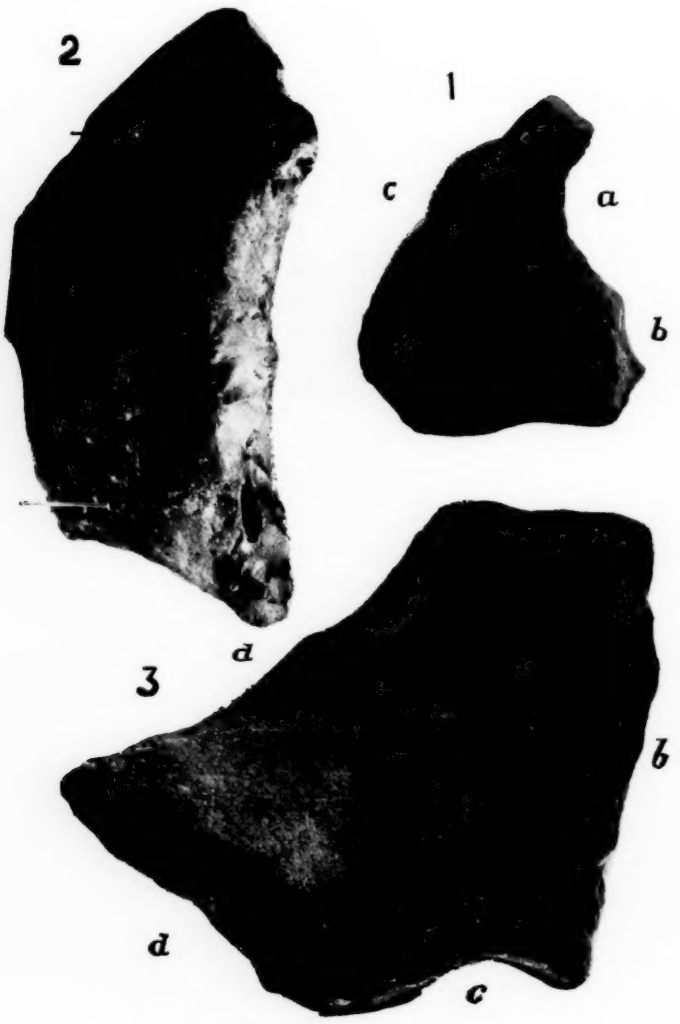
or on the upper part of the escarpment face. That the flints have been derived from the chalk is unquestioned, and the process by which they were removed from it need not be considered. During the first stage of the life history of the independent flints they were split into slabs or tablets, most of which have one flat side, and one showing the original external surface. Some of the flint slabs were flat on both sides, and they are often six or eight inches in length. The formation of these flint slabs was probably due to extreme cold, as many of the fractured surfaces resemble those of frost-flakes. Then a siliceous encrustation was deposited over the flint. The next process—leaving the chipping of the edges out of consideration for the present—was the staining of the flints to a red or reddish brown colour. The staining was no doubt due to the action of ferruginous solutions. The iron may have been derived from beds of iron sand in which the flints were once embedded, as grains of dark ferruginous sand are found still adhering to the flints in the hollows of chipped surfaces. Subsequently to the staining, the flints were scratched by some glacial agent. The striae are very abundant, and closely resemble those produced by ordinary glacial action. They were no doubt caused by the movement of pointed flints across the flat surfaces of other flints under considerable pressure. The movement of frozen masses of gravel might produce these scratches as well as the flow of dirt-laden ice, so that it is perhaps unnecessary to assume the existence of true glaciers in Kent on the evidence of these scratched flints alone.

The next change in the flints was the deposition over them of a thin layer of silex, which covers most of the chipped surfaces, and often fills up the scratches. The silica occurs in two varieties, one brown and often very fibrous, and the other white. They may have been deposited at different dates. The material is a variety of chalcedony, sometimes having the characteristic botryoidal form of that mineral.

The nature of this siliceous encrustation is not yet completely understood, but it must have been deposited by some siliceous solution similar to that which has often re-cemented shattered flints. There is, for example, in the Devizes Museum a flint that has been broken into countless fragments, many of which are as fine as grains of sand; but they are all united by a chalcedonic infiltration into a mass sufficiently solid to bear a high polish. The occurrence *in situ* of flints which have been similarly crushed and re-cemented has been recorded by Englefield and Mantell.

One feature that renders this siliceous encrustation the more interesting is that it was sometimes deposited later than one set of glacial scratches and earlier than another set.

The last process which the plateau flints have undergone is a



SO-CALLED FLINT IMPLEMENTS FROM THE PLATEAU GRAVEL OF KENT.

certain amount of polishing by the action of blown sands. Sometimes the whole surface of the flint has been thus polished, but specimens often occur in which the action has been limited to one side. Many of the flints of the Thames gravels, as, for example, at Clapham, show the same feature. In the cases of these low level gravels, the polishing was probably effected at a time of drought, when the pebbles were exposed and the river sand was dry and loose.

We therefore see that these plateau flints have been subjected to six different processes, all of which are undoubtedly natural, and each of which has left clearly recognisable traces. Taken in order of date, the processes are:—1st, splitting of the original flints into slabs, probably by frost; 2nd, a deposition of a siliceous encrustation; 3rd, iron staining; 4th, scratching by some glacial agent; 5th, deposition of a second siliceous encrustation; 6th, polishing by blown sand.

This series forms a chronological table by which we can determine the relative dates of the various chippings of the flints which are affirmed to be the work of man. I think it is quite clear from the evidence of the flints that natural agencies are sufficient to account for every splinter and scratch that they exhibit.

The first fact that tells against the artificial chipping of the flints is that the chipping is of very different dates. The process must have been continued for a considerable length of time. It is not difficult to distinguish between the chippings of different periods. The earliest fractures are the largest, as might be expected, since the conditions were the most rigorous. As the climate became milder the forces that acted on the flints became feebler, and the chips removed were therefore smaller. On some specimens it is possible to detect chippings of three different periods. The first set were struck off before the flints were coloured red; the second set broke across the margins of the first, and sometimes exposed part of the uncoloured flint below. Then came an interval during which the glacial scratches were made. These scratches cut across the surfaces formed by the first two sets of chippings. The fractures of the third set, on the other hand, are never scratched, but have themselves cut across the striae. These latest chips were small, and usually more distinctly conchoidal than the others, and in many instances they present the appearance of small frost-flakes.

As definite illustrations of the different dates of the chippings, let us examine in detail four flints which were given to me by Mr Harrison as fairly good implements (Plate IX.).

The first is shewn in Fig. 1 of the Plate.[1] It was found by Mr Harrison at South Ash, and is numbered 4997. Its size is 6 cm.

[1] For the photographs I must express my indebtedness to Mr T. H. Powell of Denmark Hill.

long by 5 cm. wide. One surface is flat and smooth, while the other is convex and rough, as about one-fourth of it is part of the original surface of the flint. From the convex side two large flakes have been forced off, probably by frost, and no doubt at about the same time as the formation of the flat face. Both sides of the flint are scratched. After the scratching a thin coating of white silica was deposited over the two large-flaked surfaces, filling up some of the scratches. And after this a second set of scratches has cut across the siliceous encrustation.

The history of the chipping of this specimen is as follows:—The curve at *a* is the result of natural forces acting on the thin edges of a natural hollow of the flint: the hollow retains the original surface, except at the edge, where three or four small chips have been forced off at a period later than the flaking. The single chip at *b* has been forced off from the other side of the flint, and probably dates from about the same period as the chips of the *a* series.

The almost straight side of the specimen (*c*) was formed by chipping at a much earlier period, before the stone was stained red, but later than the large frost flakes.

This specimen therefore exhibits surfaces of four distinct dates—

1. The original surface of the flint.
2. The flat side and the two large frost flakes.
3. The chippings on the straight edge before the iron-staining.
4. The chippings at *a* and *b* subsequent to the iron-staining.

No. 4390. A flint from Branshatch (Fig. 2) 14 cm. long by 8 cm. wide. This specimen has the original rough surface of the flint on one face and a flat frost-flaked surface; both have been encrusted by silex. The specimen is much chipped. The chips have been forced off by pressure mostly from the flat side, but at the larger end the chipping was done from both sides, forming a slight irregular ridge. The significant point about this specimen is that most of the chipping occurred subsequent to the dark staining of the flint.

No. 2711. A tablet of very dark-red flint from Rogersfield, near Ightham, measuring 15 cm. by 9·5 cm. This slab of flint was also chipped before the date of the staining, and also shews chipping by pressure from opposite sides. The chips are of different dates; the first and second sets are scratched, and the third set are insignificant and irrelevant to the main chipping. The whole surface is encrusted by silica except where it chipped.

No. Pit VI. A triangular slab of flint (Fig. 3) probably flaked from a large block by frost. The original surface occurs on the convex side and extends over a third of the edge marked *a*. Ancient chippings from opposite sides occupy the edges *d*, and some of about the same date occur along *b*. These chips were all earlier than the iron-

staining. The whole of the original surface is scratched, and the scratches sometimes extend over the edges and also cut across the surfaces left by the chips previously referred to. A dark brown, in parts fibrous, incrustation of silex thickly covers the original surface of this specimen. This was succeeded, after the glacial scratching, by a deposit of white silex filling up the hollows and scratches; the whole has been subsequently smoothed down and polished by blown sand. This specimen was dug from one of the pits in the Plateau gravel in 1896.

These four specimens illustrate the main process which the flints have undergone. They show that the chippings were not all formed at one period, a fact which it seems to me is quite inconsistent with the theory that they were artificially shaped by man. The objection seems especially convincing as, according to the advocates of that theory, all the shaping must have been done before the flints were imbedded in the gravel in which they now occur. If the flints were worked, used, and then thrown down again, we should expect to find them widely scattered over the surface as is the case with palaeolithic and neolithic implements. What possible agency could have picked them all off the surface and collected them together into this gravel bed? Further, we are told that the shaping and working of the flints by man "had taken place before the flint entered into the remarkable deposit which so altered the surface of the stone, and changed its colour into that characteristic dark-brown."[1] So according to the theory, the flints were first chipped into shape, and then carried into the plateau gravel. They were coloured subsequently, and the deposition of the siliceous encrustations, the glacial scratching, and the sand polishing all took place while the flints were in the gravel in which they now lie.

Another objection to the human working of these flints is the uselessness of the shapes into which they have been made. Flints often break naturally into a triangular form, and as the chipping has mainly acted on thin edges, abundant examples of pointed forms are found. Some of these resemble in outline the implements of later dates, but all the details of the flaking are different. Less importance is apparently placed on these triangular flints than on those with concave edges, which are supposed to have been used as flesh-scrapers. Some of the South Sea Islanders have, it is true, been observed scraping their limbs with stones; but we can hardly suppose that such vast numbers of these concave flints would have been required by the plateau folk for this purpose, especially as they would never wear out, and one would last for an indefinite time.

The chipping in some cases has not only been useless, but has even spoilt stones that might otherwise have been useful. Some of

[1] *Nat. Sci.*, April 1894, p. 259.

the specimens with sharp concave edges would have served a better purpose as scrapers if left as nature had shaped them. The irregular chippings on the edges of the natural curves has spoilt them as tools.

The vast number of the 'flint implements' from the plateau gravels is another difficulty in the supposition that they have been made by man. The 'implements' occur in an abundance described as 'marvellous' by their discoverers. We are told that two pits, dug in 1894 into a bed of gravel one foot in thickness, yielded thousands of artificial flakes and some hundreds of hollow-notched and horseshoe-shaped scrapers. The pits dug in 1896 in the same beds have yielded a similar profusion. Such results are indeed startling. Plateau man must have been a very prolific race, for his implements, almost all of one hollow-scraper type, far outnumber those of his palaeolithic successors.

In ordinary palaeolithic gravels the proportion of implements to pebbles is extremely small, and there is no difficulty in drawing a line between artificially and naturally shaped flints. But on the chalk plateau the stained flints are *all* more or less chipped. There are millions of flints on the plateaux, and it is therefore not surprising that a large number occur in which the shape resembles that of palaeolithic implements. But no distinctive line can be drawn between flints which are described as 'good implements' and others which are admittedly only naturally broken.

But if the chipping be not the work of man, what agency, it will be asked, could have produced it. Careful examination of the chipped flints soon suggests suspicious features. In the first place the chipping is limited to the edges of the slabs; there are no known instances in which the flint has been artificially flaked into the form of the weapon; the asserted human workmanship is limited to chipping of the edges of naturally-shaped flints.

Mr Harrison maintains that the chips were forced off by an agent which worked only from one face of the flint slab.[1] He regards this feature as an argument in favour of the artificial nature of the chipping. Why eolithic man should have worked only on one surface of the stone is not explained. But it is really rare to find an example that was chipped on one side only. Palaeolithic man certainly never allowed the utility of his tools to be limited by any such restriction.

The chipping was evidently due to some pressure which acted more or less at right angles to the flat surface of the flint slab. The pressure and crushing that take place during movements of frozen gravel would, it seems to me, be quite sufficient to account for all the chipping. Pebbles in the gravel would be pressed

[1] *Proc. Geol. Assoc.*, November 1893, vol. xiii. p. 162.

against the upper edges of the flint slabs and force off small flakes. Then if the flint slab be moved and inverted, renewed pressure would similarly crush the other edge, and thus account for the cases in which both edges have been chipped.

A fact which seems to me conclusive proof that the chipping was due to pressure by some yielding material from above is supplied by hollow flints. The decay of a sponge often leaves a hollow in the middle of a flint block; the edges of such hollows in plateau flints are chipped in precisely the same way as the edges. The artificial chipping of such edges by blows from another stone would be difficult, and the work would have been absolutely wasted to eolithic man. But the forcing of the finer constituents of the gravel across the hollows under the pressure from overlying material might easily have produced these crushed and apparently worked edges.

The view that the plateau flints show no sign of human workmanship I am glad to find supported by the high authority of Sir John Evans, who, in a letter of April 1896, said: "I see nothing upon them that is undoubtedly the result of human work or use; on the contrary, the rolling and wearing of the edges seem to me more probably caused by natural agencies—I see nothing but the hand of nature upon them." After a careful study of specimens lately selected as the most convincing by Mr Harrison himself, I am in absolute agreement with this opinion. And as the chipping of the flints was apparently caused by the action of extreme cold upon the gravels, movements in which were produced by the action of ice, I propose for these shaped flints the name of 'Glacioliths.'

WM. CUNNINGTON.

V

Evidence of the Antiquity of Man in East London, Cape Colony; with a Note on the Castor-Oil Plant.[1]

ABOUT the year 1857, in opening up a quarry on the left bank of the Quigney River, at its junction with the Buffalo, a shell mound was discovered, forming a rounded bluff roughly measuring 150 by 150 by 40 feet deep. The mound was clothed by 18 inches of made soil, masked by vegetable growth on the surface, and contained abundance of shells of recent mollusca (*Patella*, *Mytilus*, *Ostrea*, *Haliotis*, etc.), with bones of fish, birds, antelopes, hippopotami, and other mammalia, layers of ash, fragments of charcoal, and pieces of coarse pottery. No other implement of any kind was found, but burnt stones were very common; most of the deposit was removed to fill up a lagoon behind the East Training Wall of the Buffalo River.

The locality had remained unaltered since 1827, for in 1867 the writer accompanied the Rev. W. R. Thomson on a visit there, and heard him say that the spot was quite unchanged. The same trees and the same track remained, and but for the impediment caused by the construction of the West Training Wall of the river Mr Thomson would have undertaken to drive a bullock waggon along the same track as he had done in 1827. This track, it may be of interest to mention, ran in a straight line from where Mr A. Webb's house now stands, to the right bank of the Quigney at its mouth; then skirting the mound, it proceeded for one hundred yards along the Buffalo River towards the mouth, and from thence on the east bank it crossed the river diagonally to the ravine at the large quarry on the west bank.

So far as is known this kitchen-midden is the most recent trace of primitive man at East London, and yet must be in itself of vast antiquity.

Passing to the back of the new jail one sees a small excavation in the railway cutting, which in 1887 was covered with castor-oil

[1 The following personal observations by Mr Geo. R. M'Kay, relative to discoveries of ancient man in East London, have been forwarded to the editor by Dr Schönland. They formed part of a lecture delivered in 1887, and the editor is glad to put them on record.]

plants.[1] This was dug out for material to construct the railway embankment close by, and when undisturbed was clothed with the ordinary dense bush of the district. The section showed from above downwards 4 to 5 feet of stiff clay overlying a foot of rolled gravel, the gravel resting on decomposed rock. The gravel had all the appearance of shot of all sizes, from buckshot downwards. A little above the gravel a large number of flint implements were found, the bulk of which appeared to be "rubbers" possibly used for dressing skins or similar purposes. There were a few spear heads, some fragments of coarse pottery, and a few limpets.

This excavation is forty feet above the present level of the Quigney, and the gravel probably belongs to that river; the clay is assumed to be the accumulated wash from higher levels. The place appears to have been a workshop, for its position would afford the warmth of the morning sun with the coolness of shade in the afternoon.

On the north bank of the Buffalo, about twenty chains from the river, and just W. of the road to the Pontoon, is the residence of Mr Gately. The house and grounds stand on the rounded top of an isolated knoll, which is connected with East London *East* by a narrow neck of land. This neck is the watershed of two small water courses which unite at the S. of the knoll, and run S.W. into '1st creek.' On the top of the knoll there are from 2 to 3 feet of black mud, separated from the base rock by 1 to 2 feet of decomposed rock. The black mud is again seen to the E. of the road to the Pontoon. In this black mud on the knoll Mr Gately has found round pierced stones, stone flakes, spear heads, coarse pottery, and teeth and bones of hippopotamus. The deposition of this black mud was contemporaneous with the two water courses when they were at a level with the top of the knoll.

Fringing the whole of the south-eastern coast of South Africa there occurs in detached patches a peculiar wind-stratified calcareous sandstone. Cove Rock and Bats' Cave at East London, the 'bluff' at Natal, and the Sisters and Fountain rocks near the Fish and Kowie rivers, are conspicuous examples of the formation. At Cove Rock and Bats' Cave it abounds in fossils, especially at the latter. These consist of land and sea shells, mammalian bones, chiefly ruminants, and teeth of hippopotamus, with remains of fishes, apparently all of recent species. The rude and shifting nature of the stratification leaves no doubt that this is an aeolian formation, and comparable to that of the adjoining sandhills.

[1] An idea is prevalent that the castor-oil plant might be profitably grown as a producer of a cheap lubricant. My own experience is, that it flourishes only where rivers have cut deeply into their banks and exposed deeply seated soils, or where deep cuttings are made, or around the earth-holes of the porcupine and ant bear. It springs up on this new soil with amazing rapidity, and crowds out every other plant; but something ails it, as after a few years, whether cultivated or not, it dies down and disappears.

Out of this deposit I have myself taken three well-formed stone spear heads of the 'Moustier' type. They were at the level of low water, and in a position where they must have at one time been covered by 180 feet of the deposit. One of these is still in my possession, one is in the Albany Museum, and the third was given to a friend, who subsequently sent it to Sir John Lubbock. These aeolian deposits extend, so far as my own observations go, to a distance of certainly one mile into the ocean from the present shore. Between Sand Hill and Bats' Cave the low water platform has been cut back in many places for a width of over 200 yards by wave action. A visit to Cove Rock on a calm day will verify this if one stands on the larger of the two masses forming the rock-face on the south, for there one will see a similar platform, only at a much lower level, extending as far as the eye can follow it. The submerged reef off Nahoon Point is of this aeolian formation, and the sea has been observed to break on it upwards of a mile from the shore, while the Sisters and Fountain Rocks are really small isolated masses three-quarters of a mile from present low-water line. About one-third of the distance from the Sand Hill to Bats' Cave there is an isolated mass of this formation which becomes an island at high water; on the land-face of this mass there is a heap of shells embedded 12 feet above high-water mark. This is about 12 feet long by 3 feet thick, and contains, besides shells, fish bones and *splintered* bones of mammals; and although there are no traces of ashes or charred wood, I am satisfied myself as to its artificial deposition.

The facts concerning the aeolian deposit enumerated above leads us to the following conclusions. Since the three spear heads were lost we have to account for a depression of the land and advance of the shore-line for at least a mile, and from the average inclination of the bed of the Indian Ocean at this point, that means a land depression of over 50 feet. Then we have to allow for the gradual emergence of the land, which has enabled the sea to cut the rock back for a distance of a mile, up to its present level, which it has maintained long enough for the sea to cut a platform over 200 yards in width.

GEO. R. M'KAY.

EAST LONDON, W., SOUTH AFRICA.

VI

The Seed Production of Cut Flowers

IN the second number of the *Botanische Zeitung* for this year (Jan. 16th 1897, p. 17) Ludwig Jost brings to light a very curious fact in historical botany. He points out that at the close of 1896 H. Lindemuth has re-discovered a phenomenon, which has already twice before been described as new.

It is a well-known feature of many bulbous plants that their flowers are normally sterile, and that their reproduction takes place exclusively by the vegetative process of bulb formation. More than three hundred years ago (1577) Konrad Gesner noticed that if the flower stalks of these plants be separated from the bulb, the flowers will set their seed. This observation, however, fell into the general oblivion which overshadowed the whole of Gesner's work. In 1790 —two hundred years later—Medicus re-discovered the fact, and wrote of it in his paper "Ueber Saamenansezen an abgeschnittenen Blüthenstengeln einiger Zwiebeln und Knollengewächse" (Römer and Usteri's *Magazin für die Botan.*, vol. xi., p. 6.) He was examining the tubers of *Stellarioides canaliculta* (? *Anthericum*), and in doing so cut off the inflorescence, which he stood up in a corner of the greenhouse for the gardeners to clear away. Returning to the house a few days later he saw that the flower still remained where he had left it, and that, moreover, it was still fresh and unwithered. This interested him, and he determined to see how long it would last thus "cut off from its bulb and standing in a dry position exposed to the sun heat."

Stellarioides had been grown and flowered in this greenhouse during the three previous years without once setting seed. "I was no little surprised, therefore," he says, "to find that in due course of time the older flowers of this inflorescence, which had been separated from its bulb, formed true seed capsules." "This really remarkable and quite unexpected result," he continues, "led me at once to other experiments. For twenty years past *Amarillis reginae L.* had bloomed in this greenhouse without once setting seed; as soon as the flowers drooped, it was seen that their ovaries and all they contained withered likewise." Medicus next proceeded to cut off an inflorescence, including three flowers, and to leave this standing in the greenhouse. After a time all three flowers formed seed

capsules. The same phenomenon was seen in *Amaryllis formosissima.*

In discussing these observations Medicus writes :—"Those plants which have the property of reproducing themselves by roots, especially marked, are most unfortunate in setting seed, although no observer can deny the presence or completeness of the sexual organs. The true cause of this seems to be that these plants expend all their energy in increasing their roots and concentrate their nutritive activities on these parts, and so leave none over to contribute to the formation of seed. Annuals, or plants with a limited existence, on the contrary, for the most part, set seed, because they have little or no power of multiplying by their roots, which decay as soon as the seed is formed, and their allotted span of two to five months passed." Referring again to the complete sterility of *Amaryllis reginae*, under ordinary conditions, he adds that "scarcely, however, have we separated the inflorescence from its root, and laid it aside without moisture, than it forms large seed capsules, and clearly shows us that these would always be produced if the vigorous root formation did not rob them of all nourishment."

With the exception that we now regard bulbs and tubers as stem rather than root structures, these words have a very modern ring about them, and plainly show that what we now call correlation between the different organs of a plant was already then recognised by Medicus. One thing is very noticeable about his writings, and that is the charm of his literary style, an item which by no means graces too many of the scientific essays and memoirs of the present day.

Medicus' paper was written in May of 1790, and in the century which has elapsed since that time both Gesner's original observation and Medicus' re-discovery have been so completely forgotten that in 1896 Lindemuth published an account of the same phenomena without any idea that it had ever been noticed before. Thus, as Jost's paper in the *Bot. Zeit.* points out, we have here a fact which has three times been discovered as new, after having been twice completely forgotten. H. Lindemuth (*Berichte der deutsch. Bot. Gesell.*, pt. 7, vol. 14), after describing precisely similar facts to those which Medicus had already set down, using, however, *Lachenalia luteola* and *Lilium candidum* as his experimental objects, proceeds to recount some facts which go beyond those which his predecessors had seen. These he embodies in a second paper, contained in the same number of the *Berichte.* On 25th March he cut off forty inflorescences of *Lachenalia luteola*, and placed them in water. About three weeks later he noticed that the lower part of the stem which was under water was now curiously granulated. Here and there a granule had become larger than its neighbours,

and was easily recognised as a bulbil. At first these granules are covered by the green epidermis of the flower-stalk, but as they gradually increase in size they burst through this as little white lumps. Microscopic examination shows that these bulbils are always exogenous in their origin. Inflorescences of hyacinth which were cut off close to the bulb and placed in water then had their flowers also removed, so that nothing but the peduncle remained. When examined nearly two months later, it was found that bulbils had developed close to the places where the flowers had been situated. In this case it seems that the food-stuff in the peduncle was first cut off from the bulb, and so travelled towards the flowers, but finding its passage blocked here also by the removal of the blooms, it expends itself in forming bulbils close to their remains. Finally, Lindemuth ends his paper with practical conclusions for the culture of bulbous plants drawn from these experimental data.

This brief English note has been written in order to call the attention of those into whose hands the *Bot. Zeit.* does not usually fall to the services which Medicus rendered to plant biology. To glance through the pages of Professor Sachs' History of Botany, the only knowledge that we can gain of this older observer is in a few, scant, depreciating references. Granted that the light of genius did not lead him into the right path in one section of botany (anatomy) we still should not allow the memory of an enthusiastic and careful observer in other departments of the same science to be altogether forgotten, or, worse still, to be alone remembered for the errors into which he fell. Most of us, even to-day, are not always in the right, and this should teach us to "render the deeds of mercy" towards the memory of others who, living at a less enlightened period, sometimes went astray with their fellows, and did not rise above their times, but who on other occasions saw things with an "inward light" which was denied to their contemporaries. It is no doubt the extraordinary work of Professor Sachs himself and his school which has quite placed in the shadow all older writings upon plant physiology.

RUDOLF BEER.

SOME NEW BOOKS

The Flora of North America

Synoptical Flora of North America. Vol. I., Part I., Fascicle II. By Asa Gray, continued and edited by Benjamin Lincoln Robinson. Imp. 8vo, pp. ix. to xv., 207 to 506. New York: American Book Company, June 10, 1897. Price, 11s.

Those of our readers who are interested in North American botany will remember that Dr Gray himself published the portion of this flora dealing with the Gamopetalous orders of Dicotyledons. The two parts which appeared in 1878 and 1884 were re-issued by the Smithsonian Institution in 1886. For some time before his death Dr Gray was engaged in monographing the earlier orders of the Polypetalae, and after his death the work was continued by Dr Sereno Watson and then by Dr Robinson. The first fascicle of the present part was issued in 1895 and contained an account of the orders beginning with Ranunculaceae and, following the system of Bentham and Hooker's Genera Plantarum, ending with Frankeniaceae. The second fascicle now before us carries the work on as far as Polygalaceae. It has been printed from Dr Gray's manuscript, continued and edited by Dr Robinson, with the collaboration of Professors Trelease, Coulter and Bailey. A third fascicle to include the Leguminosae is in preparation. The work forms a concise but complete and carefully elaborated account of the flowering plants of North America (north of Mexico). The descriptions, which are sufficiently full and clear, are in English; the synonymy and bibliography of genera and species are included, and the geographical range of species and varieties is indicated. Identification of the plants is facilitated by the introduction of generic and specific keys. We congratulate Dr Robinson and his colleagues on the portion already done and wish them a speedy and successful termination of the Flora.

Mollusca

Traité de Zoologie publié sous la direction de Raphael Blanchard. Fasc. xvi., Mollusques. Par P. Pelseneer. Pp. 187, figs. 8vo. Paris: Rueff et Cie., 1897.

This, which we understand is one of the first fascicules published of what promises to be a most important and valuable work, contains, in addition to the subject announced on the title, a two-page appendix on *Rhodope*. Otherwise it amounts in fact to a second edition of Pelseneer's "Introduction à l'étude des Mollusques," to which we called attention on its publication (*Natural Science*, iv., 1894, pp. 387-388).

It is, however, so much added to and revised, that it almost amounts to a new work. At the same time, we very greatly regret to see that the useful bibliographies formerly given at the end of each section have now been omitted. On the other hand, certain omissions to which we called attention have been supplied. *Chlamydoconcha* finds a place, and some of the more important fossil families are inserted in the systematic part, which otherwise remains unaltered. A revised phylo-

genetic tree is also added. When Dr Pelseneer comes to study the gastropoda as systematically as he has done his own favourite bivalves we are convinced he will abandon the classification of the Prosobranchiata that he at present takes from Bouvier (*Ann. Sci. Nat.*, ser. vii., vol. iii., 1887), which classification is founded solely on the nervous system. He will also add some notice of *Thyrophorella*.

The principal shortcoming, however, of the work as a whole is, we think, the scanty reference to the shell, which is after all an important feature of the mollusca, and in a treatise on zoology merits a place. In this respect the work is an exception to the generality of such productions where the animal is neglected. Curiously enough it is the Pelecypod shell that is the most curtly dismissed, and this in the light of Bernard's researches is the more to be regretted.

In the development of Pelecypods, too, we miss all reference to the second origin of gill filaments, by the splitting up of a previously existing lamella, as shown to occur in *Cyclas* and *Teredo* by Korschelt and Heider, and again in *Scioberetia* by Bernard. Likewise our author appears to have overlooked the fact that the glochidial stage is not peculiar to the Unionidae, it having been found by Dall to occur in *Philobrya*. Other minor points for criticism are doubtless to be found by those who care to make diligent search for them, but the work all the same merits and will attain a high place in the estimation of those most competent to judge of it, and the praise we ventured to bestow on the first edition is yet more merited in the present one. It will be the author's fault if subsequent editions do not carry us far towards an ideal work on the subject.

It is only fair to add that a word of praise is but due to the printers and publishers for the excellent way in which they have carried out their share of the undertaking. The illustrations are, of course, those of the previous edition, enlivened in some cases by touches of colour to bring out the salient features they are intended to illustrate. There are two good indexes at the end of the part.

(BV)[2]

The Vivarium

The Vivarium, being a Practical Guide to the Construction, Arrangement, and Management of Vivaria. By Rev. Gregory C. Bateman. 8vo, pp. 424, with plates. London: L. Upcott Gill, 1897. Price, 7s. 6d.

For very many years Mr Bateman has kept living batrachians and reptiles as pets, and the beautifully got-up little volume now before us embodies the results of his experience. The work, however, is far more than a practical handbook. The author has added to his own personal observations several illustrated chapters, in which the more striking forms of batrachian and reptilian life are described in a popular manner. He thus appeals to a much wider circle of amateur naturalists than those who keep vivaria. We can thoroughly recommend the book to the general reader who desires a reliable, well-written, and non-technical account of the much neglected animals of which it treats. Our only complaint is that the illustrations are of very unequal merit, in many cases, indeed, far from accurate; and this is all the more to be regretted, since most of them are newly drawn, and might have been made admirable by a little more supervision of the artist.

Bibliography

The Theory of National and International Bibliography, with special reference to the introduction of system in the record of modern literature. By Frank Campbell. 8vo, pp. xvi. and 500. London: Library Bureau, 1896.

The main object of the present work is to demonstrate and enforce the responsibility of the government of each nation in the matter of cataloguing the literature published within its boundaries. As the author points out, it is impossible for any other body to do this work thoroughly and economically, because no other can bring pressure to bear upon the publishers. There is also this further reason for urging upon governments to discharge this obvious duty, that it is just in the department of State papers that the greatest confusion reigns and the labours of the bibliographer are most difficult. Witness Mr Campbell's imaginary, but most lifelike, conversation between a librarian and a reader in some large public library.

This main theme is treated in a series of papers which have for the most part been read before the Library Association and other bodies, and published in their journals, and in addition there are essays upon various collateral topics, such as "the influence and functions of the learned societies in regard to bibliography"—a chapter which we wish their councils would all "read, mark, learn and inwardly digest."

Mr Campbell's book is eminently suggestive, and his schemes if carried out would reduce confusion to something approaching order. With the form of his work we are not so satisfied; the plan he has adopted of reprinting essays leads to much repetition, and he often sins against his own 'theories of compilation;' but as he explains in the preface that illness prevented him from carrying out all his intentions, it would be ungenerous to dwell upon these defects, which are small in comparison with the solid value of the book.

W. E. H.

Flight and Flying Machines

The Aeronautical Annual for 1897. Edited by James Means. 8vo, pp. 178, pls. xviii. London: W. Wesley & Son, 1897. Price 5s.

The "Aeronautical Annual" for 1897 contains much that is valuable and interesting, since the contributors are nearly all of them men who are actively engaged in solving the great problem how flight may be made possible for men. First among these must be mentioned Mr Langley, who contributes an account of the experiments which, after many disappointments, ended in the manufacture of an aerodrome which actually rose in the air and continued rising and advancing for about one and a half minutes, after which it alighted rather than fell. As in the case of Mr Maxim's flying machine, screw-propellers driven by steam-power were employed. The action of the propellers is to drive the machine onward: the spreading wings have a slight upward slope, so that the force is resolved, and there is progress not only onward but upward. This aerodrome, as compared with Mr Maxim's, had the great advantage of being light, weighing, in fact, only about

25 lbs., so that experiments with it were easier and much less costly.

Mr Langley has shown, then, that flying machines can be made which will rise in the air, fly for a short time, and descend without mishap. But before the problem of flight is solved many difficulties must be got over, the difficulties of (1) launching without any elaborate apparatus; (2) maintaining equilibrium in gusty winds; (3) carrying sufficient fuel for long flights; (4) alighting safely even when the circumstances are not specially favourable. An oil engine is now being made to be affixed to the gliding machine. As an aeronaut will be on board, a distinct advance on Mr Langley's aerodrome, which had no living pilot, is contemplated.

The article on sailing flight by Mr Chanute reviews the various theories on the subject judiciously. There is no doubt, as he says, that in very many cases birds soar by the help of ascending currents of air. But it is probable that sometimes when there is no such current available, they nevertheless succeed in rising without a beat of their wings. Over level ground in Egypt, covered with green crops, where great heating or unequal heating of the surface seemed out of the question, so that there was nothing to start an upward current, the present writer has seen kites soaring with perfect ease.

Mr Chanute thinks that birds can soar by the help of a 'nearly uniform' horizontal breeze, but happily he does not, in contempt of dynamics, hold that an absolutely uniform horizontal breeze can lift them. He makes some good remarks on the question why some birds frequently soar, whereas others, and among them very good flyers, never do. He thinks that soaring depends on the arching of the wing, and it is quite possible that this may be the case. In the rapid pulsations of 'rowing' flight the front margin of the wing does the main work. In the stately gyrations of the soaring adjutant it is probable that the ample concavity is of more service. But far more cross-sections of wings than he gives are wanted before it is possible to accept this explanation, and in particular small birds should be well illustrated. It is remarkable that no bird so small as a thrush ever soars.

The subject of flight does not so often as formerly give rise to utterly wild theories. The *Annual* as a whole is remarkably free from wild writing. But the old tendency shows itself in Mr Huffaker's theory that a bird in soaring causes a down-current, which in turn starts an up-current, which supports him as he rises!

F. W. HEADLEY.

CONTEMPORARY PSYCHOLOGY

THE PSYCHOLOGY OF THE EMOTIONS. By Th. Ribot. 8vo, pp. xix+455. London: Walter Scott (Contemp. Sci. Ser.), 1897. Price, 6s.

THE NEW PSYCHOLOGY. By E. W. Scripture. 8vo, pp. xxiv+500. London: Walter Scott (Contemp. Sci. Ser.), 1897. Price, 6s.

THESE two volumes of the Contemporary Science Series exhibit Psychology in very different aspects. Prof. Ribot's work on the emotions is characterised by breadth of view, wide range of knowledge, admirable lucidity of presentation, bold and yet critical use of

hypothesis, and a distinctly genetic motive. The subjects with which it deals are of necessity vague in outline and indefinite in limitation. The social and moral feelings, the religious and aesthetic sentiments, the emotions associated with intellectual endeavour—all these present us with such a variety of factors, such an interlacement of the threads of our mental life, so much overlap and so much that is dependent on individuality of character, that anything like mathematical exactness or precision of measurement is at present, and is likely long to remain, impossible.

On the other hand, Dr Scripture's *New Psychology* is characterised by limitation of field, a devotion to exactness of measurement, a love of mathematical and formulated presentment, an impatience of hypothesis, and a refusal to look beyond the formulated facts. The subjects with which it deals are carefully restricted to those which lend themselves to physical measurement. The new psychology—a remarkably unsatisfactory and somewhat arrogant title—deals with a comparatively small area of the field of mental endeavour, and one in which there is no luxuriant profusion of mental products. But it endeavours to deal with this small area with an exactness and precision which is in itself wholly praiseworthy. And if the results attained by the large expenditure of time, money, and energy in the well-equipped psychological laboratories across the Atlantic would seem at present scarcely commensurate with the cost, this will not, we trust, damp the ardour of enthusiasts like Dr Scripture. Psychology is a great subject of which we are only just beginning to realise the importance. There is plenty of scope both for the breadth of treatment we find in Prof. Ribot and for the patient experimentation of those whose work is described in the *New Psychology*.

A Blind Guide

Nature-Chat. By Edward A. Martin, F.G.S. 8vo, pp. 141. London: R. & A. Taylor, 1897. Price, 1s.

There are numbers of folk who derive much innocent enjoyment from natural history, and who have a harmless enthusiasm for 'Nature.' No one would wish to interfere with their amusement; but the matter assumes a different aspect when their trivial observations and inaccurate assertions are obtruded upon the public. This is the case in the latest addition to the *biblia abiblia* which are now so freely issued from the press—"Nature-Chat," by Mr Edward A. Martin. Mr Martin is a leading light of the Selborne Society; he guides the members in their summer rambles, and lectures to them on winter evenings. He has written a bibliography of White of Selborne, and seems to think he follows the methods of 'Gilbert,' as he affectionately styles him.

Disraeli said that the critics were men who had failed in literature; Mr Martin makes it clear that a teacher may be one who has never succeeded in learning. He has brought together in this volume a number of paragraphs, many of which, we believe, have appeared in a local magazine or newspaper; others are "parts of letters addressed to friends," who must, we think, be somewhat bored by the honour of Mr Martin's correspondence. We look in vain for a single addition to knowledge in this collection of trivialities.

The following paragraph may be quoted as exemplifying at once Mr Martin's scientific attainments and his literary style:—

"To be thoroughly acquainted with the beautiful grasses of Great Britain is to possess a knowledge of which I am ambitious. As I was collecting grasses on my walk, I pulled some concerning which I was at a loss to know whether they were of identical species, or whether they were distinct. Almost as the thought passed through my mind, my eyes lighted upon a grass of which the lower half of the blossom was fully expanded. This showed the features of one specimen, whilst the upper part, which evidently was yet to expand, showed the features of the other. Thus Nature answered her own problem which she had put to me." (P. 34.)

We are tempted to ask why Mr Martin has not acquired the "knowledge of which [he is] ambitious"? The number of common British grasses is not large; every manual contains their description. But it is only too clear that Mr Martin has not even a slight acquaintance with grasses, for he talks of 'the blossom' when he means the inflorescence, just as he speaks of a 'specimen' when he means a species. And what was the plant after all? He tells us that "Nature answered her own problem," but does not give us her reply. We feel inclined to imitate Nature in "putting a problem" to Mr Martin —What useful purpose can be served by printing paragraphs of this kind?

Mr Martin made the strikingly original observation that the flowers of the everlasting pea turn blue when fading. He then "watched the creeper closely, with the result that it has borne blossoms which were blue in the first place." This curious consequence of Mr Martin's vigilance leads him to say—"the seeds it will be well to collect": but he was counting his chickens too soon, for on p. 77 we read: "In a former letter I referred to the blue blossoms of the everlasting pea which had appeared in my garden. Neither of them have been succeeded by the usual pods of seeds, so that I shall not have the satisfaction of rearing seedlings from them, as I had anticipated." Can Mr Martin suppose that this kind of thing adds to our knowledge?

But the author rises to higher flights than these. "You know," he says, "the *yucca*, which is said [inaccurately, but this Mr Martin does not know] to flower but once in a hundred years. Then comes such a burst of brilliance that it requires a period equal to that which allows our orb to roll its ponderous body along its tremendous path around the sun, a hundred times or thereabouts, in order to recover its flowering energy. What an act of self-denial is this: what an act of self-immolation, in order that its duty may be fulfilled!" The only parallel to this reflection is one which occurred some years since in the catalogue of a picture exhibition in South London—"Scene in Ceylon: Elephants bathing. How much the elephants in the Zoo have given up for our sakes!"

The book is full of inanities and ineptitudes, and the literary style is in harmony with the subjects discussed. If a fly tumbles into the milk-jug, Mr Martin speaks of its 'unwelcome last sad bath.' Dead nettles are 'magnificent' and 'gorgeous.' Certain flowers smell like 'a glass of sherry'; two plants in the same paragraph 'rejoice in' their names. The author speaks of his 'legal brother,' as if he had another

brother who was not legal, though we think he only means to say that the gentleman in question is a solicitor. And what are we to make of the following sentence:—"We are so matter-of-fact in our science—too matter-of-fact—to thoroughly appreciate the science which has become science, become what is known (*scio*, I know) by the labours of the great men of the past, who alone, perhaps, when discovering, were able fully to realise the poetry of their own discoveries." It is perhaps our own fault, but we are unable 'to thoroughly appreciate' either Mr Martin's facts or his style.

JOHN HUNTER

JOHN HUNTER: Man of Science and Surgeon (1728-1793). By Stephen Paget. With Introduction by Sir James Paget. 8vo, pp. 272. London: T. Fisher Unwin, 1897. Price, 3s. 6d.

MR STEPHEN PAGET'S small volume on John Hunter inaugurates a new series, entitled "Masters of Medicine," edited by Dr Ernest Hart, and published by Mr Fisher Unwin. It is not a technical work appealing merely to the medical profession; it is a well-written and highly entertaining account of one of the greatest students of biology in the last century, full of interest to the general scientific reader. The volume does not contain much new matter; but Mr Paget has spared no pains to go to the original sources for information, and he has made good use of the letters and records preserved in the Royal College of Surgeons, London.

John Hunter, as a great pioneer in biology, is perhaps too much neglected by the present generation. His infusion of purely scientific methods into the profession of surgery led to results of such moment that his labours in other directions are apt to be overshadowed and forgotten. Those, however, who are familiar with such of his biological and geological writings as were recovered and edited by Sir Richard Owen in 1861, can estimate the old surgeon at his true worth; and it is of no little importance that his memory should be kept green in the minds of those who are now following in his footsteps. Mr Paget's delightful chapters can scarcely fail in this purpose, and we urge all who have not yet realised the extent and bearing of John Hunter's researches and the influence of his personality, to read the new biography at once. His favourite maxim was—"Don't think, try; be patient, be accurate." The story of his life will be found inspiriting by any plodding student.

PENGELLY

A MEMOIR OF WILLIAM PENGELLY, of Torquay, F.R.S., Geologist, with a Selection from his Correspondence. Edited by his daughter, Hester Pengelly. With a Summary of his Scientific Work, by the Rev. Professor Bonney. 8vo, pp. x. 341, portrait and 10 illustrations. London: John Murray, 1897. Price, 18s.

NOT only the numerous personal friends of William Pengelly but also all who are interested in the progress of our knowledge of the antiquity of man and allied subjects, will be glad to learn that Miss Hester Pengelly has published an account of her father's life. The handsome volume of over 300 pages which we have just received, contains

an ample selection from his correspondence arranged in chronological order, with paragraphs welding them into a connected story; and this is followed by an interesting chapter by Professor Bonney on the general character and value of his scientific work.

Pengelly's genial personality was so widely known and beloved, not only by men of science but by many devoted to other intellectual pursuits, that his biography will find no lack of enthusiastic readers. The story of his early life as a Cornish sailor; of his career as a schoolmaster at Torquay; of his growing fame as a geologist, his discoveries of fossil fish-remains in the Devonian rocks of Cornwall, and so forth; of his share in the exploration of Brixham cavern in 1858; and finally, of his great work in arranging and personally superintending the excavation of Kent's cavern—all this is told in an entertaining manner in his letters. The various little incidents in his career are recalled as we read, and those who knew the man himself will recognise his characteristic traits and modes of expression. We have only one criticism to offer, namely, that far too much of the correspondence is printed. A considerable proportion of it is very trivial and ephemeral, of no interest to anyone except his immediate family, for whose private view alone it was intended. Some other letters, we think, are inserted with rather questionable taste, as, for example, one in reference to a candidate's touting for the fellowship of the Royal Society on p. 282. Biographers ought to distinguish between strictly personal matters and those bearing upon the progress of a life's work. The latter alone are worthy of being preserved in a memorial volume.

Professor Bonney's summary of Pengelly's researches, occupying only thirty pages, is an admirable contribution. It is both concise and clear, and written in a style readily comprehensible to the general reader. The first section deals with the geologist's first serious undertaking, the investigation of the Tertiary deposits of Bovey Tracey; the next part is concerned with the examination of the caverns, in association with which the name of Pengelly will be longest remembered; and the third or final part includes a number of miscellaneous geological studies of the south-western district of England. We will not attempt to analyse this chapter; it must be read in its entirety to be appreciated.

Miss Pengelly's biography appropriately concludes with a list of her father's writings, more than 100 in number, and it is prefaced by a very successful copy of A. S. Cope's portrait of Pengelly, which was painted in 1882.

The Vertebrate Skeleton

The Vertebrate Skeleton. By Sidney H. Reynolds, M.A. 8vo, pp. xvi. 559, with 110 illustrations. Cambridge: University Press, 1897. Price, 12s. 6d.

The latest volume of the Cambridge Natural Science Manuals (Biological Series) is a useful small compendium of osteology by Mr S. H. Reynolds, Lecturer and Demonstrator in Geology and Zoology at University College, Bristol. For the most part the work is a laborious compilation, which sometimes rather detracts from its interest and value; but the actual descriptions of certain typical

skeletons are based upon personal observation and research, while the clear diagrammatic illustrations are nearly all refreshingly new, many of them taken from the beautiful preparations in the central hall of the British Museum (Natural History), others from specimens in the Cambridge University Museum of Zoology.

The plan adopted by the author is to give first an account of the general skeletal characters of the group of which he is treating, with the characters of its several sub-divisions; secondly, to describe in detail the skeleton of one or more selected types; and thirdly, to treat the skeleton as developed in the group in question, organ by organ. The account of each type skeleton is made complete in itself, so that the elementary student can, if he wishes, use the book merely as a laboratory guide to the few leading forms of skeleton to which he ordinarily confines his attention.

The author is, of course, a teacher, and he presumably knows the requirements of his students; but we are inclined to think that the handbook he has produced is far from well-arranged for practical purposes. The information is admirable, usually up-to-date, and not often faulty—though a work of such wide scope must necessarily have its imperfections; but there are endless repetitions as we turn over the pages, the facts concerning a single structure or phenomenon are sometimes inconveniently scattered, and there is a lack of some fundamental idea to unite the various parts of the work into one harmonious whole. The facts of embryology may sometimes be of doubtful import, and our present knowledge of palaeontology may encourage many fanciful notions and speculations. But if both these aids to formulating a scheme be rejected, there is still the good old-fashioned method of Comparative Anatomy, which (in our opinion, at least) is more useful for teaching purposes than the disconnected mode of treatment in the handbook before us. We have noted similar want of coherence in Cambridge biological teaching before. Since the days of Francis Maitland Balfour, the philosophy of the subject seems to have become gradually neglected, while the dry facts have been more and more constantly presented in unattractive array. Mr Reynolds is likely to have the opportunity of revising his manual in a new edition very soon—for it fills a decided gap, and will be helpful to many who have hitherto been compelled to turn to numerous and varied abstruse treatises for guidance. We would therefore urge him to consider these important points, and render his work more worthy of the great labour he must have bestowed upon it.

Two New Editions

Lessons in Elementary Biology. By T. Jeffrey Parker. Third edition. 8vo, pp. xxiii. 503, with 127 illustrations. London: Macmillan & Co., 1897. Price, 10s. 6d.

Elements of the Comparative Anatomy of Vertebrates. Adapted from the German of Dr Robert Wiedersheim. By W. N. Parker. Second edition, founded on the third German edition. 8vo, pp. xvi. 488, with 333 illustrations. London: Macmillan & Co., 1897. Price, 12s. 6d. net.

These two text-books by the brothers Parker are too well known and widely appreciated to need any recommendation here. It suffices to record the publication of a new and revised edition of each of them.

Prof. Jeffrey Parker's "Elementary Biology,' in its new form, differs from the preceding editions in the increased attention devoted to the higher animals and plants. The general chapter on the higher animals has been expanded into four beautifully-illustrated chapters, dealing respectively with an outline-classification, the starfish, the crayfish, the fresh-water mussel, and the dogfish. The additional botanical matter refers in the same style to *Equisetum*, *Salvinia*, *Selaginella*, the Gymnosperms, and the Angiosperms. These supplementary sections will indeed, as the author himself remarks, contribute much to the usefulness of the book.

Prof. Newton Parker's second English edition of "Wiedersheim" may almost be described as a new book. The original descriptions and arrangement are retained as far as possible, and most of the old figures are reproduced, although a few have been replaced and others added. Prof. Wiedersheim has also revised the whole. But to bring the work up to date, and at the same time not increase the size of the volume, it has been necessary to abridge much of it and recast other portions; while the useful bibliography in the appendix has been considerably expanded to increase its usefulness to advanced students. We only notice one serious blemish, namely, the unreliable character of some of the references to the skeleton in extinct animals. The figures of the pelvic arch of *Plesiosaurus* on p. 115 are quite erroneous, and might easily have been replaced. "*Labyrinthodon ruetimeyeri*" is certainly not a labyrinthodont. The wing-finger in the Pterodactyles is not the fourth but the fifth digit. Before the next edition we would suggest that this section of the work be submitted to someone skilled in vertebrate palaeontology for revision.

Scraps from Serials

Mr Adrien Dollfus has paid considerable attention to the terrestrial Isopoda, the wood-lice of the world, and has published a paper in the October number of *La Feuille des Jeunes Naturalistes* showing the wide range of some of these little animals. Considering the mode of life of these Isopoda, it is not at all surprising to find certain species have a wide distribution, but it is interesting to have this distribution put on record. The species dealt with are *Armadillo murinus*, Brandt; *Armadillidium vulgare*, Latr.; *Porcellio scaber*, Latr.; *P. laevis*, Latr.; *Metoponorthus pruinosus*, Brandt; *Ligia exotica*, Roux. Of these five the *Metoponorthus* has the widest range, being recorded for America (N. to S.), Azores, etc., the whole of Europe, Africa (N. to S.), Asia (E. to W.), and Australia (N. Caledonia and Marianna Islands); *Porcellio laevis* is the next most widely distributed, having much the same range as the *Metoponorthus*, but found also in the Bermudas, Melbourne, and various islands of Oceania, and restricted to the N. of Africa. A sketch map accompanies the paper, which has an additional value in that it mentions the collections in which the particular specimens recorded are to be found.

The *American Journal of Psychology*, vol. viii. No. 3, contains a 'Study of Apperception,' based on experimental work on the reading of words by Dr Pillsbury. Typewritten words photographed on lantern slides were projected on a ground-glass screen, and exposed for two-

tenths of a second. They were mis-spelt by the omission of a letter, the substitution of a wrong letter, or the blurring of a letter by printing an x over it. The object was to determine the relative influence of the objective factor in the visual stimulus, and the subjective factor through association and apperception. The experiments were, so far as possible, carefully tabulated, and it required not a little skill (and some imagination) to educe results of any decisive value. A good *resumé* of Prof. Wundt's views on apperception is given, and the conclusions reached are in line with those obtained in a quite different way by Dr Stout in his recent work on *Analytical Psychology*.

The New Age, which was started in April, is published on the 15th of each month by the proprietor, 68/2 Shikdar Bagan Street, Calcutta. The annual subscription is Rs. 2.12. The editor is S. C. Mukhopadhaya, M.A. There is some want of discrimination shown in the selection of tit-bits for this "Journal of universal information," but it will probably be of interest to the readers for whom it is intended, and it interests us as an expression of the views of the educated Hindu community.

We regret to learn that with the October number the *International Journal of Microscopy and Natural Science*, for sixteen years the organ of the Postal Microscopical Society, has ceased to exist from lack of adequate financial support.

The *Psychological Review*, vol. iv. No. 5, contains a suggestive paper by Prof. Mark Baldwin on the "Psychology of Social Organisation." The author is one of those who are in sympathy with biological studies, and who seek to correlate the biological and the psychological factors in the development of social life from its pre-social beginnings. The same writer gives in the "Princeton Contributions to Psychology," reprinted from the preceding number of the *Psychological Review*, a discussion of "Determinate Evolution," which should prove of interest to biologists.

In the *Albuquerque Morning Democrat*, Prof. Cockerell gives a report on the Mexican dietary as studied by Prof. Goss. He finds that the principal food of the Mexican peasant is flour, corn meal, and chili, and that he is using more carbo-hydrates and less proteids than is desirable. The Mexican gets most of his proteids from frijoles, and for the better nourishment of the peasant Prof. Goss suggests a larger use of frijoles in proportion to the flour and meal. The work is being carried on at the Mesilla Park experiment station, and the object aimed at is an improved dietary after examination of soils and improvement in agricultural produce.

In the *Transactions of the Perthshire Society of Natural Science*, vol. ii. pt. 5, Colonel Duthie writes on the British abode of the Crested Titmouse (*Parus cristatus*); Messrs Coates and Macnair on a banded Hornblende Schist at Balhoulan Quarry, Pitlochry; and Mr Macnair on Rocks of Highland Perthshire. The president, Mr Coates, also gives a presidential address entitled The Origin of Soils, with special refer-

ence to the soils of Perthshire; making altogether a good geological part of the *Transactions*.

Timehri for June contains Nesting of some Guiana Birds, by C. A. Lloyd; Tobacco and Cotton Cultivation in the British West Indies, by W. H. Burnley; Result of Recent Scientific Researches into the Agricultural Improvement of the Sugar Cane, and on other Sugar Cane Experiments, by J. B. Harrison.

New Serials

The Institute of Jamaica has now begun to issue *Annals* in addition to its well-known *Journal*. We have received the first part containing a list of the Decapod Crustacea of Jamaica by Mary J. Rathbun of the U.S. National Museum.

The *Archives de Parasitologie*, under the direction of Raphael Blanchard, is a new review announced to appear in Paris next January. The parts will be published at variable intervals, each containing about 160 pages, and four will constitute a volume, price 30 francs. The journal will comprise both original articles and reviews of progress.

The *Archives of Skiagraphy* has changed its title to *Archives of the Roentgen Ray*. It is edited by W. S. Hedley and Sydney Rowland, and is published by the Rebman Co. The illustrations are chiefly of medical and surgical interest.

Further Literature Received

Vorlesungen über Bacterien, A. Fischer: Gustav Fischer. An Introduction to Geology, W. B. Scott: Macmillan. Traité de Botanique, L. Courchet: Ballière. The Mathematical Psychology of Gratry and Boole, M. E. Boole: Sonnenschein. In Northern Spain, Hans Gadow: Black. Catalogus Mammalium, fasc. iii., E. L. Trouessart: Friedländer. Darwin and After Darwin, III. Post-Darwinian Questions, G. J. Romanes: Longmans. Familiar Wild Flowers, F. E. Hulme: Cassell.

An Address on Acquired Immunity, G. Archdall Reid: *Lancet*. Notes on the Coccidae, T. D. A. Cockerell: *Rev. Mus. Paulista*. On the Nature of the Röntgen Rays, Sir G. G. Stokes: *Manchester Lit. and Phil. Soc.* Harvard University, Dept. Zoology, 1897-98.

Timehri, June; Amer. Geol., Oct.; Amer. Journ. Sci., Oct.; Amer. Nat., Oct.; Ann. Inst. Jamaica, Vol. i., No. 1; Annot. Zool. Japan, Vol. i., Part iii.; Botan. Gaz., Sept.; East Asia, Oct.; Feuilles des Jeunes Nat., Oct.; Irish Nat., Oct.; Journ. School. Geogr., Sept.; Knowledge, Oct.; Literary Digest, Sept. 11, 18, Oct. 2, 9; Naturae Novit., Sept.; Naturalist, Oct.; Nature, Sept. 23, 30, Oct. 7, 14; Nature Notes, Oct.; Naturen, Sept.; New Age, Sept.; Photogram, Oct.; Rev. Scient. Sept. 18, 25, Oct. 2, 9; Science, Sept. 10, 17, 24, Oct. 1; Sci. Amer., Sept. 11, 18, 25, Oct. 2; Scot. Geogr. Mag., Oct.; Scot. Med. and Surg. Journ., Oct.; Victorian Nat., Aug.; Westminster Review, Oct.

OBITUARIES

WILLIAM ARCHER

BORN MAY 6, 1830. DIED AUGUST 14, 1897.

WILLIAM ARCHER, who died in Dublin on August 14th last, was born on May 6th, 1830. He devoted himself for many years to the investigation of the lower plants and animals, especially the Desmids and certain groups of the Rhizopods and Infusoria. From 1876 till 1880, he acted on the editorial staff of the *Quarterly Journal of Microscopical Science*, in which most of his important work was published. Many of his valuable papers, however, were issued by the Dublin Natural History Society, now extinct, whose proceedings are, unfortunately, very scarce. He was an original member, and for many years secretary of the Dublin Microscopical Club. His eminence as a microscopist led to election into the Royal Society in 1875. In 1876 he became librarian to the Royal Dublin Society, and when the bulk of the collection was transferred to the Government to form the National Library of Ireland, Archer became head of the new institution. His later years were busily occupied in the duties of this office, and he laboured unremittingly in the transfer of the books to new quarters, and their arrangement and cataloguing on the Dewey system, of which he was an enthusiastic advocate. Two years ago he was compelled to retire, having reached the age of sixty-five. His familiar figure will be sadly missed among Dublin men of science, whose respect for his wide learning was accompanied by hearty admiration for his personal worth. G. H. C.

The following deaths are also announced :—KARL VOGEL and WILHELM LIEBENON, eminent German cartographers ; KARL WILHELM PETZOLD, the physical and astronomical geographer ; J. H. TRUMBULL, philologist and member of the National Academy of Sciences, U.S.A. ; ERNEST HUTH, professor in Frankfort and well known as a populariser of science ; EMIL SCHMIDT, a teacher of zoology in Berlin ; EDGAR MACLURE, professor in the Oregon State University, recently killed by a fall on Mount Rainier, which he was exploring with a party ; at Port Antonio, Jamaica, Dr J. E. HUMPHREY, associate professor of botany in Johns Hopkins University, Baltimore ; C. S. ROY, professor of pathology in the University of Cambridge, aged 43 ; AUGUST MOJSISOVICS, professor of zoology and comparative anatomy in the University of Graz ; Dr HOLMGREN, professor of physiology in the University of Upsala, aged 66 ; and Dr WELCKER, professor of anatomy in the University of Halle.

NEWS

THE following appointments are announced :—Dr Lehman Nitsche to be keeper of the department of Anthropology in the La Plata Museum, in succession to Dr Ten Kate ; Dr P. Zwaardemaker to be professor of physiology in the University of Utrecht ; Dr Carl Zelinka to be professor of zoology in the University of Czernowitz ; W. L. Bray to be professor of botany in the University of Texas ; H. L. Jones to be associate professor of botany in Oberlin College ; Dr Hans Reusch, director of the geological survey of Norway, to be Sturgis-Hooper professor of geology in Harvard University, for the season 1897-98.

WE have had occasion to refer to the good work that has lately been done in the Bootle Museum. The Committee did well when, some three years ago, they engaged Mr H. C. Chadwick as museum assistant. We therefore regret to learn that they can no longer afford to retain his services.

THE new Museum of the Brooklyn Institute of Arts and Sciences was opened early in October, when President Eliot of Harvard delivered an address.

DR ALEXANDER HILL, Master of Downing College, has been elected Vice-Chancellor of the University of Cambridge.

THE Eighth International Geological Congress is to meet in Paris in 1900, the year of the Exhibition. The visit to Vienna, which was to have taken place in that year, is postponed to 1903.

REUTER announces that Nossilor has arrived at Tiumen from the Kara Sea. He has explored the Yalmal peninsula, and discovered a shorter waterway between Siberia and Europe, and one free from the sea ice.

IT is proposed to decorate the Zoological Park at Washington with bronze groups of Indians and wild animals. Mr Edward Kemeys will probably receive this commission, which will put on record many vanishing types of animal life.

THE *Botanical Gazette* announces that the Smithsonian Institution has appointed a commission, with Dr V. Havard as chairman, to collect information concerning the medicinal qualities of the plants of the United States of America.

MR R. C. CHRISTIE has presented to the Owens College, Manchester, his share of the estate of the late Sir Joseph Whitworth, estimated at about £50,000. The fund is to be devoted to a new building with which the name of Whitworth can be associated.

ACCORDING to the *Athenæum*, it is proposed to establish at Swansea a branch University College in association with either Cardiff or Aberystwyth. This town already possesses an important library and museum in the Royal Institution of South Wales.

THE Academy of Sciences of Berlin has granted a sum of 3000 marks to Prof. B. Hagen, Frankfurt, for the publication of an anthropological atlas ; 1500 marks to Prof. Kohen, Greifswald, for mineralogical researches ; and 800 marks to Prof. R. Bonnet, Greifswald, for anatomical work.

Science states that plans have already been made for the new building of the American Geographical Society, New York, although the site has not yet been

decided upon. The Society owes its present flourishing condition to Judge Daly, who has been its president for thirty-three years.

A COLLECTION of horns of mammals made in South Africa by Mr J. Rosen is now being exhibited on loan in the Brighton Museum. Most of the known species are represented, and many of the specimens are remarkably fine. One pair of horns of a koodoo measure 47 inches in length.

THE Louisiana Society of Naturalists was founded on July 22nd. It starts with forty-five members under the presidency of Prof. J. H. Dillard of Tulane University, the secretary being Mr E. Foster. According to *Science* the Society will establish a museum and library, and will publish proceedings.

THE Museum of the Perthshire Society of Natural Science is progressing favourably, though hampered by want of funds. We trust the town authorities will help in the matter after so much energy has been spent in getting things into shape. The Society's library has received a set of the 'Challenger' Reports from the Government.

NEXT year Mr J. E. Spurr will lead an expedition to Alaska to make a further survey of the gold resources. $5000 has been appropriated by Congress for the purpose, but according to *Science* an effort will be made to increase the appropriation to $25,000 in order that a complete survey may be made and a geological map of the region prepared.

DURING the present session Prof. Boyd Dawkins will give a series of twelve short addresses on geological subjects in the Manchester Museum. These are to be delivered alternately on Saturday and Sunday afternoons. We are glad to note that the Museum has just received a donation of £1500 from Mr Edward Holt towards the building fund.

ACCORDING to the *American Journal of Science*, the Geological Survey of Canada has recently acquired a mass of meteoric iron from Thurlow, Hastings Co., Ontario. It is an irregularly-shaped, truncated pyramidal mass, with a more or less rectangular base, measuring 0·16m. by 0·135m., and weighs 5·42 kilos. It is to be named the Thurlow meteorite.

THE International Ornithological Congress will meet this year at Aix on November 9. The International Congress of Zoology will meet at Cambridge on August 23, 1898. Sir William Flower has issued a circular letter asking for co-operation with the general committee in raising a fund to defray the necessary expenses of the meeting. The committee has just met in London to arrange the preliminaries.

PROF. MICHAEL FOSTER has been delivering lectures in Baltimore during October, and he now proceeds to deliver a course of Lowell Lectures at Boston. In the middle of the month, Dr Nansen passed through London on his way to America, where he has many engagements. *Science* announces that at the close of his first lecture at New York a medal will be presented to him by the American Geographical Society.

AT the annual meeting of the Hull Scientific and Field Naturalists' Club, held on September 29, it was reported that great progress had been made during the year. Thirty-eight new members had been elected, and the attendance at meetings had increased by 50 per cent. The Society wisely arranges its programme so that lectures of general scientific interest alternate with its more technical and original local work.

THE Trustees of the British Museum have been approached by the Council of

the British Association with regard to the establishment of a Bureau of Ethnology for Greater Britain. If this arrangement can be come to there is no doubt that the information obtained would be of great service to science and utility to the Government. Sir John Evans, Sir John Lubbock, Mr C. H. Read, and Prof. E. B. Tylor made the report which was placed before the British Association.

Sir Joseph Hooker has finished the "Flora of British India," begun twenty years ago, and has received, according to the *Kew Bulletin*, a despatch from the Government of India, through Sir George Hamilton, recognising his services to India in cordial and sympathetic terms. Sir J. Hooker has, we are also glad to learn, offered to undertake the preparation of the remaining volumes of the late Dr Trimen's "Handbook to the Flora of Ceylon," and the necessary material and specimens have already arrived at Kew from Peradeniya.

Among the Russian geologists who took part in the Oural excursion, none endeared himself more to his fellow-travellers than the young candidate in Natural Science, L. Spendiarow. He died suddenly of heart disease almost immediately after the return to St Petersburg. His father has given to the International Geological Congress the sum of 4000 roubles, the triennial interest of which is to be awarded by the President of each Congress as a prize for the best geological work done during the preceding three years.

We learn that the Government have presented a set of the 'Challenger' reports to certain local scientific societies. This is a very excellent stimulus, no doubt, but we hope the familiar notice that is to be found inside the Record Office publications deposited in our Free Libraries is to be found also in these scientific reports. Local societies exist by the enthusiasm of the few, and when they fall into decay such volumes might well be taken from them and passed on to another centre where they will be more appreciated.

The Bulgarian Government has received by bequest from Eulogius Georgieff, the founder of the Sofia University, the sum of 20,000,000 francs for public purposes. This includes 6,000,000 francs for a technical school for Sofia. The University of Lyons will devote 42,000 francs to complete the biological laboratory of Tamaris, near Toulon, and will probably endow it to a moderate extent. Indianapolis will receive fifty-six acres of land for a botanical garden and ornithological preserve from Mr W. W. Woolen. Yale University has received $5000 by the will of Miss Julia Lockwood for the foundation of a scholarship.

The University Extension Lectures in London for the coming session were listed by *The Echo* on October 5. The following may interest some of our readers :—The Geography of Britain and the British Seas, by H. J. Mackinder, at Gresham College ; the World's Great Explorers, by H. Yule, Oldham, at Toynbee Hall ; Evolution and Darwinism, by E. O. Paskyn, at Lewisham ; Physiology of Plants, by E. O. Paskyn, at Morley College ; the Earth, by F. W. Rudler, at Croyden and Toynbee Hall ; Our Common Minerals, by F. W. Rudler, at West Ham ; and Human Anatomy, by Chalmers Mitchell, at Toynbee Hall.

Miss Kate M. Hall, Curator of the Whitechapel Museum, has made arrangements to co-operate with the teachers of the elementary schools in the district in demonstrations to their classes when they visit the Museum. As is well-known, the Code of 1895 allows visits to be paid during school hours under proper guidance to museums, art galleries, and other institutions of educational value, such visits being counted as "attendances." At Whitechapel each headmaster or mistress is invited to bring one assistant and forty-five pupils on each occasion. The pupils are divided into three groups, the teachers and Miss Hall each taking one. Such specimens as may be removed from the cases are arranged on three tables.

Each group spends a quarter of an hour at each table, and the rest of the hour is devoted to questions or to a general survey of the Museum.

THE meeting of the Botanical Society of America, which was held at Toronto at the time of the visit of the British Association, seems to have been a great success. Dr J. M. Coulter was the president, and there were present a large gathering of English and Foreign botanists. N. L. Britton of New York was chosen president for 1898, which session will be held in Boston, an invitation from the Missouri Botanic Garden for the spring of that year having been reluctantly declined. The chief papers were—A case of ecblasteris and axial prolification in *Lepidium apetalum*, by B. L. Robinson; Movement of protoplasm in coenocytic hyphae, by J. A. Arthur; Pollen grains and antipodal cells, by J. M. Coulter; The transition region of the *Caryophyllales*, by F. E. Clements; A revision of the species *Picea* occurring in North-Eastern America, by D. P. Penhallow; Bibliographic difficulties, by E. L. Greene; and the botanical gardens of Jamaica, by W. Fawcett.

CORRESPONDENCE

INHERITANCE OF ACQUIRED CHARACTERS

In view of the doctrine which constitutes the corner-stone of Prof. Weismann's theories of heredity—the non-inheritance of acquired characters—any exceptions to the rule which he lays down may be worth notice. I desire to submit to your readers for consideration one which has appeared to me to bear upon this question—viz., the direction in which the hairs slope on the extensor surface of the forearm in certain hairy quadrupeds and in man. The course which the hairs take on this small area varies in different animals, but, as far as I can learn, in two main directions only. In the first of these, which one may look upon as more 'normal,' the slope is on the whole directed in the axis of the limb towards the distal extremity, and is thus in keeping with the general slope of hair on the other areas of this and the posterior extremity of the animal, as in the great Ungulate order and a few species of monkeys. The second type of direction presents a certain reversal of the slope—viz., that the hairs after a manus-ward course on the flexor surface curve round the lateral borders of the fore-arm, and when they reach the extensor surface they pass in a reversed direction, in some animals on the whole area, in others on the proximal portion only, towards the trunk. This second type is seen clearly in man at all ages, in the anthropoid apes, especially in the long-haired Orang, to an extent exceeding any other animal I have seen, in most lower monkeys, and in the Carnivores. In the last-named order the direction is visible in the terrestrial Carnivores, Arctoidea, Cynoidea and Aeluroidea, especially in the shorter-haired forms, but in those with longer hair on the limbs the general set of hair on this area is towards the trunk, even if indistinct in some. It is the second of these types to which I would draw attention as being a departure from the first, which is seen in Ungulates, and which is more in accordance with the natural arrangement of hair. In this great group of animals, the Ungulates, there is a very general habit of flexing the fore-limb in the attitude of repose, as one sees commonly in a herd of cows and horses grazing, when these limbs lie doubled up and the hoofs are resting under the fore part of the trunk. The same position can be observed in other families of this order in confinement. A similar slope of hair is also seen in certain Marsupials, Kangaroos, for example; and in these animals, which almost always lie on their sides when at rest, the position would be indifferent as to influence upon the slope. In Ungulates the pressure thus exercised when the limb is in acute flexion would act at right angles to the axis of the limb, and, except in so far as it would confirm the original slope of the hair on the extensor surface, it would be indifferent in its effect on the hair-slope. But in the Primates, Carnivores, and certain other animals in which the surface of this limb-segment in question is exposed to pressure acting in a different manner, one is not surprised to find a different slope of hair. In Man it is frequently subject to pressure against some underlying fixed surface. In apes and monkeys of all kinds it is extremely common in their sitting posture to see their upper extremities strongly flexed at the elbow and resting against their lower limbs. In terrestrial Carnivores the normal attitude of repose is, except on the occasions when they lie asleep stretched out on their sides, that the fore-limbs are planted in front of the trunk, seen most noticeably in the 'couchant' position which they commonly assume. In all these instances it is obvious that there is a slowly acting mechanical force in the downward and forward direction by reason of the weight of the limb itself, and the fore part of the trunk which is supported by it. The effect of this pressure would be to cause the hairs to slope towards the trunk, as is found to be the case in these animals. There are of course many apes and monkeys, notably the chimpanzee, gorilla, and orang, in which the same direction is taken by the hairs on other aspects of the same limb on regions where no pressure can influence it. In these the general set of hair towards the trunk is probably due in part to the effect of gravitation on the long hair, and perhaps, as Mr Wallace has suggested, to the influence of heavy rain in tropical forests when the hair would act to the body as thatch does to the roof of a house.

The direction of hair-slope on this area is, of course, congenital; it is therefore a primary character, not one acquired by the individual through secondary forces. It is congenital in the human infant, in the young monkeys and young Carnivores which I have been able to examine, and presumably it is so in all in which it is found.

The consideration of this point leads us to the conclusion that the character before us is one which it is impossible to look upon as having any 'survival-value,' or as due to any form of selectional processes within the germ acting under adaptive requirements. An adequate secondary cause is suggested for its original production in the ancestors of those which now exhibit it. Perhaps someone more qualified to speak could determine whether or not this is one of the exceptions to Prof. Weismann's rule, the non-inheritance of acquired characters.

WALTER KIDD.

12 MONTPELIER ROW,
BLACKHEATH, S.E.

INSECTS AND FLOWERS

WITHOUT at this time attempting to discuss the general arguments used by Mr Bulman in his interesting paper in your August number, I should like to draw attention to the following paragraph (p. 103):—

"Again, if our native flowers are the result of the selective action of our native bees, and those which they have specially chosen for countless generations, how is it that bees take so readily to many flowers of very different forms introduced into our gardens from abroad? For such introduced plants are in many cases freely visited by native bees."

I think we need more information about this matter. I have found, here in New Mexico, that garden flowers do not as a rule attract many species of native bees, unless they are very closely related to native flowers. A flower garden is nearly always disappointing as a hunting-ground, the bees found there being mainly certain widely distributed types which visit very many species of native plants. In a luxuriant garden at Santa Fé the best collecting is on the weeds, not at all on the cultivated flowers.

Solidago canadensis is a native plant very common in New Mexico. It is also grown in gardens in Europe, and a small list of common flies visiting it there is given by Hermann Müller. In Las Cruces, on Aug. 30 of this year, Prof. C. H. T. Townsend was sweeping *S. canadensis* for flies. He swept at the same time a lot of hymenoptera, which he handed to me. I have sorted out the fossores and bees, and here is the list:—

ANTHOPHILA

Agapostemon melliventris, Cress. ♂.
,, *radiatus*, Say. ♂.
Panurgus rhodoceratus, Ckll.
Colletes americana, Cress.
Halictus stultus, Cress. vel peraff.
,, *ligatus*, Say.
,, sp.
Nomia nevadensis, Cress.
Melissodes agilis, Cress. ♂.
Perdita sphaeralceae, Ckll., one ♂.*
Epeolus lunatus, Say vel peraff.
Podalirius maculifrons, Cress.

FOSSORES

Microbembex monodonta, Say.
Philanthus ventilabris v. *frontalis*, Cress.
,, sp.
Steniolia duplicata, Prov.
Oxybelus quadricolor, Ckll. and Baker, ♂.
,, *sparidcus*, Ckll.
,, *abdominalis*, Baker. ♂. (new to N. M.).
,, sp.
Plenoculus cockerellii, Fox.
Stizus godmani, Cam.
Aphilanthops laticinctus, Cress.
Cerceris acanthophilus, Ckll.
Myzine frontalis, Cress. M. S. ♀.
,, *hyalina*, Cress. ♂.
Anacrabro boerhaaviae, Ckll.
Paratiphia, sp.
Scolia, 2 spp.

I have on former occasions done equally good collecting from the *Solidago*. Who can show a similar list in Europe collecting from the same plant? I doubt if it can be done, yet the plant belongs to a European genus, and is much less specialised for insects than many others.

T. D. A. COCKERELL.

MESILLO, NEW MEXICO, U.S.A.,
Sept. 3, 1897.

* I suspect that this came from a *Sphaeralcea* growing among the *Solidago*, especially as there are in the lot two beetles which breed on *Sphaeralcea*. These may, however, have strayed from their normal plant.

WOMEN versus BIRDS

In *Natural Science* for August (p. 77), the question is asked as to what more we can do besides attempting to influence women in the home-circle so as to prevent their barbarian slaughter of the birds for decoration of themselves. I think we could do a great deal more, and will instance this by an account of what occurred here some years ago. A Frenchman settled down in the neighbourhood of Mount Ophir in Malacca as a collector of bird skins for the trade. He employed a large number of native hunters, and the slaughter of our most beautiful birds—sun-birds, trogons, fairy bluebirds, and many others—was horrible. However, the Government, on learning this, passed an ordinance forbidding the killing of birds in the colony without a licence. The man then moved to one of the native states out of colonial jurisdiction, but the law was immediately introduced there. Then it is said he sold the goodwill of the business to an unsuspecting native and disappeared. The native, of course, on attempting to carry on the business was arrested. Since then our birds have returned to their desolated haunts and are as plentiful as ever, and no further attempt has been made to establish the trade in this country. Indeed, I can hardly remember a case where it has been found necessary to put the law in force, though, of course, a few birds are now and then illegally killed by shooters. Of course we are much assisted by the licensing of guns. Natives, for excellent reasons, are only allowed gun licences for purposes of defence against tigers, robbers, etc., or to keep away wild pigs or other destructive animals, and this checks bird-killing very well.

In Borneo, under British rule or influence, the Mias is also protected, no person being allowed to kill one except by special permit, only granted for scientific purposes. And licences to collect orchids are also issued there to check the wholesale destruction of these plants.

The forests of the Dindings, especially one jungle round a hill called Gunong Tungul, are tenanted by rhinoceroses, which the natives used to trap in pitfalls. As the animal is perfectly harmless, and a very interesting beast, the District Officer, Mr W. C. Mitchell, endeavoured to prevent its destruction, and though the Government did not see their way to legislating to save it, he succeeded in preventing any more from being killed. The Rhinoceros is considered by the Chinese as a very valuable medicine, or rather collection of medicines. They pay large sums for its carcase, and dry and prepare every bit of it to ship to China. This was the only inducement to the Malays to kill it. Thanks to this action there are still plenty in this district, where I have more than once come across them, or heard them gallop off snorting like a pony.

Now, for the trade in bird-skins in colonies or countries under British influence there can be no excuse. It can and should be stopped by local ordinance. The difficulty lies in countries not under our control, such as Mexico, Brazil and New Guinea. In nearly all these places, however, there are naturalists, and often Natural History Societies. It should be the duty of all scientists in these countries to bring pressure to bear upon their Governments to check or prevent the trade in bird skins. Part of New Guinea is under British influence, and any destruction of Birds of Paradise here could be prevented. The naturalists of Holland and Germany might also be asked to approach their Governments on the subject, and to stop the trade in their Colonies.

In the list of birds destroyed given in the article in *Natural Science*, it is not altogether easy to guess from what part of the world the skins and plumes are derived. Some are evidently Indian—*e.g.*, Peacock, Indian Parrots, Impeyan Pheasant, &c.; others Tanagers and Humming Birds from South America. Let our naturalists at home first trace up the sources of the supply, and then those in the countries whence the skins come can be appealed to to bring the matter before their respective Governments. This plan, though it will probably not entirely stop the trade, will at least save the birds of some corners of the world, and will strike a blow at the fashion which I trust it may never recover.

H. N. Ridley.

Singapore, *April* 25, 1897.

AUSTRALIAN NATURAL HISTORY

As an Australian worker I trust that you will allow me to protest against the hostile criticism of an Australian student, as such, by a London authority, as such, on p. 5 of this volume. Criticism of Australian work upon its merits will always be welcome, but when a reviewer is invoked to write down a paper because if a certain arrangement "was not made it ought to have been" then fair play is disregarded and the honest aims of criticism made subservient to less worthy ends. For it is evident that under ordinary circumstances the brilliant writer of your editorial would not have stooped to crush a weaker brother on the score of a few misprints, and the difference which will ever exist in their view of what constitutes a 'species' between a 'splitter' and a 'lumper.'

More than a year ago three naturalists gathered each a miscellaneous collection from a Pacific Atoll; two of these went direct to London, the third to Sydney. Not a word has as yet been written on the material sent to England. A generation since Darwin complained, in sorrow and surprise, that he could not secure specialists to work out the results of his South American journey. To-day boundless wealth of material pours into London, but what proportion of it is ever studied? There is no lack in London of material, of Pacific material, even of Funafuti material for students with an appetite for work: yet is it touched? But if a local student makes an honest attempt to further the cause of science, a bitter cry arises from the British Museum specialist—defrauded of his rights!

J. Douglas Ogilby.

Livingstone Road, Petersham, Sydney,
25th August 1897.

FUNAFUTI

In the first paragraph of your review of "Australian Museum Memoir III. (on Funafuti Atoll), part 2," which appeared in your July number, is a statement of your impression that a stipulation had been made for the Royal Society to have the right of prior publication, and that if such an agreement was not made it ought to have been.

I am directed by the trustees of the Australian Museum to put you in possession of the actual facts of the case; and, as your statement has been made public, to request that this also might be given the same prominence.

On 7th April 1896 the Local Committee in Sydney, representing the Royal Society of London, asked the trustees to nominate an officer to accompany Prof. Sollas on the Expedition to Bore a Coral Reef, and Mr Charles Hedley was appointed. The trustees were informed in a letter, signed by the Chairman of the Local Committee, that "in regard to the secondary objects, that is, the Collection of Specimens of Natural History, each member of the expedition will be at liberty to retain or exchange anything he may obtain. The expedition as an undertaking, therefore, does not interfere with the collections of [Mr Hedley]. Your trustees are thus at liberty to impose what conditions they think best upon their representative in this respect." In addition to this, the question of publication was raised at a meeting of the Local Committee, at which one of the trustees of the Museum and the curator were present as well as Prof. Sollas, and the reply was that no restriction would be placed on it.

It will thus be seen that, in taking part in the expedition, a stipulation was made beforehand for the right of publication of the results obtained by the trustees' representative.

S. Sinclair,
Secretary to the Trustees of the Australian Museum.

Sydney, *8th September* 1897.

[We deeply regret that information which we had from two independent sources, believed to be authoritative, should have led us to make any suggestion of unfairness on the part of our friends and fellow-workers in Sydney. A correction of our error was published before the various Australian protests reached us. The rest of our remarks, whether of praise or blame, remain absolutely unaffected by this correction.—Ed. *Nat. Sci.*]

NOTICE

To Contributors.—All Communications to be addressed to the Editor of Natural Science, at 67 St James' Street, London, S.W. Correspondence and Notes intended for any particular month should be sent in not later than the 10th of the preceding month.

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